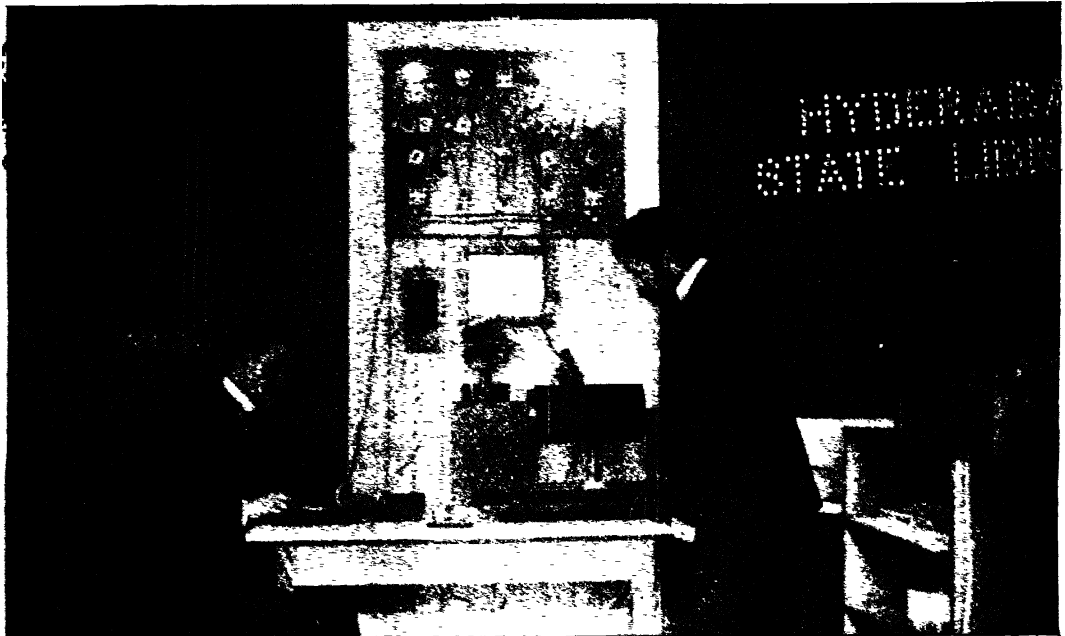
THE CONTEMPORARY SCIENCE SERIES.

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THE NEW PSYCHOLOGY.



AN EXPERIMENT ON RHYTHMIC ACTION.

THE NEW PSYCHOLOGY.

BY

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TO THE FOUNDER OF THE FIRST PSYCHOLOGICAL LABORATORY,

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Professor of Philosophy in Yale University;

THIS BOOK IS DEDICATED

IN RECOGNITION OF THEIR INVALUABLE SERVICES

IN ESTABLISHING A NEW SCIENCE.

PREFACE.

During the last few years a wide interest has been aroused in the growth of a science of mental life which employs methods hitherto peculiar to the physical sciences. The interest has been followed by misconceptions of the most varied character. Some have supposed the new science to concern itself with experiments on thought-transference and clairvoyance; others have regarded it as a presumptuous sub-department of the physiology of the senses and the brain; and still others have treated it as merely a materialistic philosophy in one of its aberrations.

Amid such confusion it is no wonder that people ask: "What *is* the new psychology? Is it brain-physiology, or spiritualism, or a new kind of metaphysics?" And the chemist, geologist, or physicist is also inclined to ask: "Is it a science at all?"

My aim in this book is to show just what the new psychology is. I have sought to make clear the fundamental ideas of the science, and have not attempted to cover the whole field of investigation. Salient points and fundamental methods were chosen rather than a multitude of details.

The reader will probably notice a difference in the treatment of Parts II., III., and IV. In Part II.—Time,

my aim was to illustrate methods of careful experiment and measurement in psychology; in Part III.—Energy, I emphasised the results obtained rather than the means; and in Part IV.—Space, I have given a systematic development of principles rather than particulars either of methods or of facts. The development of the standards of time (Chap. V.) from a purely mental standpoint was suggested by Jevons's chapter on time; the standards of space (Chap. XXV.) were treated analogously. The third fundamental quality, energy, has been placed in a clear light on the psychological side by Ostwald's essays. A consistent treatment, however, would have required more rearrangement of psychological data than I felt it wise to attempt at present; therefore, after stating the principles regarding energy (Chap. XII.), I have in the rest of Part III. kept to the usual classification according to quality and intensity.

It is, perhaps, unnecessary to state that the approximate method of computing the mean error has been used on p. 47 and elsewhere in order to avoid a tedious explanation; moreover, for most purposes the approximation is amply satisfactory.

It becomes my agreeable duty to acknowledge the extensive and varied assistance received from others in preparing this book. I am under special obligations—too numerous and varied to be detailed—to Professors Wundt, Binet, Sergi, Cattell, Fullerton, Baldwin, Ladd, James, Stanley Hall, Sanford, Delabarre, Krohn, Lombard, and Calkins, to Drs. Dearborn, Bolton, and A. Moore, and to Mr. Lough. Thomas Edison, Esq., of Orange, N.J., was kind enough to send the strip of kinetoscope negative used in Chap. VI. Thanks are due to Leopold Voss, of Hamburg, publisher, for the use of the figures illustrating Heymans's experiments

in Chap. XXVIII.; and also to Georg Brokesch, of Leipzig, for the portrait of Fechner. The picture to illustrate the runner's reaction-time was made from a lantern slide loaned by Rector Geo. Fox, of New Haven, Conn.; the original photograph from which it was made is no longer accessible, and it is unfortunately impossible to assign full credit to the photographer who has unintentionally given such a remarkable illustration of a psychological fact.

Figures 1, 2, 3, 8, 17, 49, 51, 92, 93, and 117, are reproduced by permission directly from the original articles cited in the text; figure 18 is from a print by Ottomar Anschütz, Posen; figures 9, 45, 46, 53, 56, 74, 79, 81, 82, 83, 84, 85, and 86, have been newly drawn or cut after the originals cited in the text; a number of the illustrations are from my "Studies," references being given at the appropriate places.

Many photographs of apparatus were received from various American laboratories, but unfortunately only a few of them were so made that they could be used for reproduction by photo-engraving. I particularly regret my inability, on account of late arrival, to use more than a small portion of the manuscript and photographs kindly prepared by Professor Delabarre (Brown University), and by Mr. Lough and Dr. Dearborn (Harvard University).

To Professor George M. Duncan, of Yale, I owe the deepest gratitude for a careful criticism of every important point in the book. From Dr. C. E. Seashore, my friend and assistant, I have received many valuable suggestions and much aid in preparing both manuscript and proof.

E. W. S.

YALE UNIVERSITY, 1897.

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THE NEW PSYCHOLOGY.

PART I.

METHODS.

CHAPTER I.

OBSERVATION.

The development of a science consists in the development of its means of extending and improving its method of observation. The great step that has lately been taken in psychology lies in the introduction of systematised observation, by means of experimental and clinical methods. This change is one which the physical sciences have long since undergone, but which occurred in psychology only a few decades ago. Probably nothing could make clearer the point from which the new psychology takes its departure than Bacon's picture, if applied to psychology up to a short time ago :—

"The sciences to which we are accustomed have certain general positions which are specious and flattering, but as soon as they come to particulars, . . . when they should produce fruit and works, then arise contentions and barking disputations, which are the end of the matter, and all the issue they can yield. Observe

also, that if sciences of this kind had any life in them, that could never have come to pass which has been the case now for many ages—that they stand almost at a stay, without receiving any augmentations worthy of the human race, insomuch that many times not only what was asserted once is asserted still, but what was a question once is a question still, and instead of being resolved by discussion is only fixed and fed; and all the tradition and succession of schools is still a succession of masters and scholars, not inventors and those who bring to further perfection the things invented." [1]

Strange as it may seem, the novelty of the new psychology results largely from the practical adaptation of a principle for whose application Bacon was so earnest. This principle can be summed up as a deep distrust of man's mind when left to itself, but a firm belief in its reliability when working in true comradeship with carefully determined facts.[2]

We have now before us the point at issue between the old method and the new, namely: is simple observation of our minds adequate to the establishment of facts concerning mind?

The first criticism on unaided observation is that it gives us only general outlines of facts. Let a dozen persons pay a visit to Berlin; each one is to write a book on the subject. On some main facts all will agree, *e.g.*, the existence of Friedrichs-Strasse, the plentifulness of the soldiers, and the cleanness of the streets. In many respects they will disagree, although all may have seen exactly the same things, *e.g.*, the good temper of the inhabitants, the sensibleness of the house-numbering, and the efficiency of the police.

The next criticism is that unaided observation falsifies to a greater or less extent what it states as facts,

[1] Bacon, "Instauratio Magna," Preface.
[2] Ibid., "Novum Organon," bk. i.

and that it is therefore unsatisfactory as a method of attaining accurate and trustworthy knowledge.

If we consider the vagaries of the human mind even under the most careful control, we see at once why this falsification must occur.

Every observation must in a certain sense be true, for the observing and recording of an event is in itself an event. But before we proceed to deal with the supposed meaning of the record, and draw inferences, we must take care to ascertain that the character and feelings of the observer are not to a great extent the phenomena recorded.[1]

The chief sources of untrustworthiness of observation can be stated as (1) the error of prejudice, and (2) the error of unconscious alteration.

The error of prejudice is a most dangerous one. The mind of man is like an uneven mirror, says Bacon, and does not reflect the events of nature without distortion. "It is difficult to find persons who can with perfect fairness register facts for and against their own peculiar views. Among uncultivated observers the tendency to remark favourable and forget unfavourable events is so great, that no reliance can be placed upon their supposed observations. Thus arises the enduring fallacy that the changes of the weather coincide in some way with the changes of the moon, although exact and impartial registers give no countenance to the fact. The whole race of prophets and quacks lives on the overwhelming effects of one success, compared with hundreds of failures which are unmentioned and forgotten." "Men mark when they hit, and never mark when they miss."[2] We should do well to bear in mind

[1] Jevons, "Principles of Science," p. 402, London, 1887.
[2] Ibid.

the ancient story of one who was shown a temple with pictures of all the persons who had been saved from shipwreck after paying their vows. When asked whether he did not now acknowledge the power of the gods, "Aye," he answered, "but where are they painted that were drowned after their vows?"

"The human understanding," says Bacon, "is no dry light, but receives an infusion from the will and affections, whence proceed sciences which may be called 'sciences as one would.' For what a man had rather were true he more readily believes. Therefore he rejects difficult things from impatience of research; sober things, because they narrow hope; the deeper things of nature, from superstition; the light of experience from arrogance and pride, lest his mind should seem to be occupied with things mean and transitory; things not commonly believed, out of deference to the opinion of the vulgar. Numberless, in short, are the ways, and sometimes imperceptible, in which the affections colour and infect the understanding."[1]

Our passions, our prejudices, and the dominant opinion of the day are abundant sources of dangerous illusion, by exaggerating the probabilities in their own favour and in depreciating the contrary probabilities. The vivid impression which we receive from present events, and which causes us scarcely to remark the contrary facts observed by others, is one of the principal causes of error against which we cannot be too much on our guard.[2] Habit and sympathy determine to a great extent our beliefs, and, we may add, our statements concerning our observations.

It might be said that this all refers to common

[1] Bacon, "Novum Organon," bk. i., Aphorism xlix.

[2] Laplace, "Theorie Analytique des Probabilités," Preface, p. cii, Paris, 1820.

observers—but it is, alas, true of scientific people also, even men in the laboratory.

An observer is in general disposed to ignore a result as being erroneous that is in contradiction with an expected result or with other apparently good results. "It is my firm opinion that no man can examine himself in the most common things having any reference to him personally or to any person, thought, or matter related to him, without being made aware of the temptation to disbelieve contrary facts and the difficulty of opposing it. I could give you many illustrations personal to myself about atmospheric magnetism, lines of force, attraction, repulsion, &c."[1] It is a fact of my own experience that the most difficult thing to learn and to teach does not lie in the manipulation of apparatus or the execution of experiments, but in the art of truthfully recording results and stating conclusions.

Turning to the source of error spoken of as "unconscious alteration," we are tempted to say that the source of all error is prejudice, and the result is unconscious alteration. This would, however, extend the word prejudice beyond its usual meaning of tendency to liking or disliking. By the error of unconscious alteration we wish to indicate those cases where no prejudice, in the usual sense, is to be assumed.

I say, says Mill, that I saw my brother this morning. The actual observation consisted of a combination of patches of colour. From these I conclude that I saw my brother, *i.e.*, I conclude that they were like those patches which I had previously seen and which I was accustomed to call my brother. I might have seen some combination

[1] Faraday, "On Mental Education," London, 1853; also in Faraday, "Experimental Researches in Chemistry and Physics," p. 463, London, 1859.

so similar that I was mistaken. I might have been asleep and dreamed the sensations, and yet have confused the dream with actual experiences.[1] In all these cases there were real observations, *e.g.*, the patches of colour sensations. What was wrong was the adding of others and calling the whole an observation. It frequently occurs to me that I dream of some experience, and then a day or so afterwards am uncertain whether I actually had the experience or not. It often occurs on receipt of a letter that I think out the answer while going along the street. Then when I have time to write the answer I forget whether I actually wrote it or only thought about writing it. This has occurred so often that I have had to adopt a definite criterion: if I find the letter in a certain pigeonhole it has not been answered, otherwise it has.

"If in the simplest observation, or in what passes for such, there is a large part which is not observation, but something else; so in the description of an observation there is, and always must be, much more asserted than is contained in the perception itself. We cannot describe a fact without implying more than the fact."[2]

Another kind of unconscious alteration is unconscious omission. Our travellers in Berlin undoubtedly did see a great deal, but there was much valuable information that escaped them. They noticed and recorded only a small portion of what their eyes saw, and while taking notes on the shop windows in the Kaisergallerie, they were blissfully unconscious of the watchful policeman around the corner. Yet this policeman's eye is more characteristic of Berlin than all the shops put together.

With such dangerous sources of error ever present

[1] Mill, "Logic," bk. iv., ch. i. § 2. [2] Ibid., ch. i. § 3.

in our observations, it seems justifiable to conclude that mere observation is not adequate to completely establish the facts of mind. Unaided observation can be trusted only for rather vague, cursory, and one-sided views of phenomena.

Having thus been driven to the conclusion that unaided observation is an inadequate method, we must find a new method. Observation in general consists in paying attention to events. How can it be improved and rendered more accurate?

Improvement in the method of observation may be made by use of statistics, experiment, or measurement, or by combinations of the three. By taking *statistics* on *numbers* of persons we might determine if all persons had a green-blue after-image for a red colour. By taking *statistics* on the *same* person for different times of day, for different conditions of health, &c., we could settle the question of permanency of the relation of the after-image to the original colour. By using various hues of red, orange, &c., we might gain *experimental* basis for the statement that the colours can be arranged in a closed curve in such a manner that the colour of the after-image shall be found at the opposite end of the diameter drawn from the original colour. By combining *statistics* with *experiment* we might prove that the form of this curve is not the same for all persons. If now we introduce *measurements* with the colour-wheel or the spectrophotometer, we can determine the exact form of this curve with relation to all possible colours. By *statistical measurements* we can gather precise information on mankind down to the minutest detail that our apparatus and opportunities will allow us to seize.[1]

[1] It is true that to large and important portions of mental science we cannot yet apply the improved methods, and must still rely on

These methods of improving observation, however, presuppose the possibility of observing mental phenomena. This is an assumption that has been made throughout the chapter. The assumption has been seriously, earnestly, and violently questioned, and has been vigorously, emphatically, and pugnaciously supported. In fact, the question concerning the possibility of observing mental phenomena has been the source of one of the queerest quarrels in the history of science. This is the great feud concerning the validity of "introspection."

What is "introspection"? It can be briefly defined as "looking into one's own mind." Stated in other terms, observation is the directing of attention to phenomena of any kind, and introspection is observation of a class of these phenomena.

Now let us first consider some of the famous objections urged against "introspection," or observation of our mental life.

"Introspection mutilates the facts of consciousness even in contemplating them, tears them from their necessary connections, and hands them over to a tumultuous abstraction." [1]

"The first difficulty in psychological observation arises from this, that the conscious mind is at once the observing subject and the subject observed. What are the consequences of this? In the first place, the mental energy, instead of being concentrated, is divided, and divided in two divergent directions. The state of mind observed, and the act of mind observing, are

what has been aptly termed "descriptive psychology." Nevertheless, the last few years have seen the application of experimental methods to such apparently inaccessible problems as hallucinations, emotions, the thoughts of the insane, &c.

[1] Herbart, "Lehrbuch zur Psychologie," § 3.

mutually in an inverse ratio ; each tends to annihilate the other. Is the state to be observed intense, all reflex observation is rendered impossible, the mind cannot view as a spectator ; it is wholly occupied as an agent or patient. On the other hand, exactly in proportion as the mind concentrates its force in the act of reflective observation, in the same proportion must the direct phenomenon lose in vivacity, and, consequently, in the precision and individuality of its character.

"In order to observe, your intellect must pause from activity ; yet it is this very activity that you want to observe. If you cannot effect the pause, you cannot observe ; if you do effect it, there is nothing to observe."[1]

The attempt is sometimes made to save the validity of introspection by saying that we observe the facts by means of memory, not at the instant in which we perceive, but the moment after, and that this is, in reality, the mode by which we acquire the best of our knowledge of intellectual actions.

Unfortunately memory, even immediate memory, is untrustworthy for more than crude outlines. Unaided observation was crude enough ; so-called "reflection," or introspection by memory, is still cruder. All the distortions of any method of observation, even "introspection," are added to the errors of memory.

This claim for "reflection" was apparently the last argument for introspection. It has been said to be so manifestly valueless that people have sought to throw overboard not only "reflection" and "introspection," but all observation of ourselves. This is the standpoint of the older physiological psychologists.

Comte, for example, claims for physiologists alone

[1] Comte, "Positive Philosophy," ch. i.

the scientific knowledge of intellectual and moral phenomena. He totally rejects psychological observation properly so called, *i.e.*, observation of the internal consciousness.

Again, we hear: "Every study of psychology, whose object is the exact description of facts and research into their laws, must henceforth set out with a physiological exposition, that of the nervous system. This is the obligatory point of departure, not resulting from a passing fashion, but from nature itself, because the existence of a nervous system being the condition of psychological life, we must return to the source, and show how the phenomena of mental activity graft themselves upon the more general manifestations of physical life." [1]

The final state of the quarrel leaves two parties: the observationalists asserting the validity of introspection, and the psycho-physiologists demanding its replacement by the alone trustworthy method of psycho-physiology. Is there any hope of adjusting such a difference?

In the first place, can agreement be found concerning the validity of introspection? Let us consider what takes place in any observation.

In observing objects we introduce a changed condition of affairs. Whatever we pay attention to becomes a more prominent object than the rest of our experience. After we have observed a particular flower the fields are not the same to us as before; even when observing it we entirely overlooked many things we would otherwise have noticed. Or, while observing meteors in one region we overlook what is happening in another region. Again, after-images play a very small part in the lives of those who have never attended

[1] Ribot, "English Psychology," p. 198.

to them; but while observing these images the ow[illegible] neglect everything else. In general, we may say that the act of observing introduces a change in the sum total of experience; the more intentionally and systematically we observe, or the more carefully we experiment and measure, the greater the distortion and change produced.

The act of observation, then, apparently introduces an error; the error, however, is not in the observation, but in the conclusion drawn. Observation gives us information regarding the fact observed without regard to its relations to simultaneous or successive facts. The flower, the meteors, and the after-images were observed for themselves alone; for our particular purposes the flower was the only thing growing in the fields, the meteors were the only things in the heavens, the after-images the only things we saw. But if a conclusion as to the general constitution of the fields, of the heavens, or of our vision, be drawn from the particulars mentioned, the conclusion is necessarily wrong. We were not observing fields but flowers, not the sky but the meteors, not the whole visual space but the after-images; to observe the larger units we would have to direct our attention differently.

Turning to introspection, we find the case exactly similar; introspection *does* distort things and lead to erroneous conclusions, but so does all observation. The objections to introspection apply just as completely to physical or botanical observations as to psychological ones. I observe the sparks from an electrical machine or the flower in the field, and utterly overlook the machine itself, or the other plants in the field. In fact, I cannot help distorting and mutilating what I observe. If I wish to carefully observe the construction of the machine, I must neglect the spark; if I wish to study

the tree, I neglect the flower. Likewise, if I observe a memory, I overlook an emotion; if I study my despondent condition in one way, I neglect it in another.

These difficulties are inevitable in any science; they are necessary consequences of the method of observation. The blunder of the critics of "introspection" lies in assuming that the results gained in a particular case by a particular procedure for a particular purpose could be supposed to represent the whole state of affairs. It is as if they said, "Any observation of the flower in that field must be worthless, because by so observing it you utterly distort and falsify the actual conditions prevailing in the field." Of course the reply is: "If you wish the whole field observed we will take proper steps to observe it. Indeed, even if you wish the whole field observed with the same carefulness as that flower the problem is not impossible, although with the time and means at present disposal it may not be profitable or practicable."

Thus on the question of the validity of introspection we have granted to both parties the main contention—namely, that it is a valid method, and that it is in a degree erroneous. The other contention was the improvement of methods. The psycho-physiologists claimed that an entirely new method, with experiments and measurements like those of the natural sciences, must be found; the observationalists declared that any attempt to reduce mental life to brain processes was utterly absurd. Can we find an adjustment here?

At the present day I think we can yield to the observationalists the claim that mind and brain are not exactly the same thing. Perhaps mental phenomena can be present only when certain changes occur in the nervous system, but an idea is not explained if we know that at the time of its occurrence a certain change occurred in

a certain part of the brain. Suppose I have a feeling of pain. It may be that before, at the time, or afterwards, some change has occurred in my brain; it may be that such a feeling is always accompanied by a definite nervous change; but is there any intelligible meaning to the statement that observation of the nervous change is the same as observation of the pain? It is the same with all states of mind: under the closest investigation they always remain states of mind; I can never resolve them into motions of particles of matter. The two sets of phenomena may be inseparably connected and parallel—that is still a problem to be solved—but it is evident that mental phenomena exist as mental phenomena; and that, therefore, there ought to be a science of mental phenomena as distinguished from the science of bodily phenomena. Approaching the question from the other side, we must arrive at the same result. It may be that at some future time an anatomist can so accurately examine the brain with a microscope that he will be able to say with surety, "This person had such and such sensations, such and such memories," &c., but he can attach meaning to these statements only by calling up the phenomena to which they correspond in his own mind. A deaf investigator could never imagine what sensations of sound are, even if he could see and record all physical and physiological phenomena that accompany them. He who had such a perfect knowledge of the finest and most complicated movements in the brain that at each moment he could tell the position of every molecule, would, in spite of this, not find there pleasure or pain, memories or volitions.

Functions of the brain may correspond to or may hold some other relation to mind, yet mind and brain are not the same; and observation of brains is not observation of sights, sounds, pains, volitions, &c.

Thus the great bone of contention has been yielded to the observationalists without going beyond what, I think, any psycho-physiologist of to-day must at once agree to.

How about the claim on the other side, that the old method must be replaced by a new one? This can be granted to the psycho-physiologist. A new method, a natural-science method, has been found; it is the method of psychological experiment.

It might long ago have been foreseen that some blunder must be the cause of the strife between the introspectionists and the psycho-physiologists. The cause of the strife can be removed by yielding to both parties just what each was really striving for. The introspectionist would maintain a true science of mind apart from physiology; and rightly so. No physiological experiments or methods can ever reveal a mental act. The psycho-physiologists were animated with the desire for truly scientific work to replace the inaccuracy of the introspectionists; and rightly so. The new psychology gives both what they wish: a purely mental science founded on careful experiment and exact measurement.

CHAPTER II.

STATISTICS.

ROUGH, general observation being insufficient for our science, it becomes necessary to devise methods of aiding observation. One method of doing this is to make the observations in large numbers and to count the proportion of agreements to disagreements. If a thousand persons observe the same thing, or if one person makes observations and obtains the same result a thousand times with not a single contradiction, the observation can be considered to be better established than if the number of contradictions is equal to the number of positive assertions.

We count the number of times we perceive an object out of the total number of opportunities for perceiving it; or we count the number of persons who can perceive the object; or we count both the number of times for each person and also the number of persons.

The method of counting agreements and comparing the results constitutes the method of statistics.

It has long been observed that some regularity appears in the actions of men. Would it not be possible to gain some valuable knowledge from a study of the relation of the number of times an act was committed to the number of times it was not? This thought has led to considerable activity in col-

lecting statistics concerning the mental characteristics of man.

But statistics do not consist simply in the collection of figures. Scientific counting is something more than setting off one number against another. The statement that 50 % of a group of persons are subject to hallucinations may be a useful and important fact, but it is not sufficient for scientific deductions. Qualitative statistics involves in addition to the actual counting a consideration of the purposefulness and practicability of the plan of observation, the manner of carrying it through, the reliability and availability of the results, the combination of the judgments, and the critical justification of the conclusions. Statistics, says Meitzen, is the method of gaining judgments and conclusions concerning the relations of a mass of changing phenomena too numerous for a single general view, by counting characteristic qualities among them; or, it is the method of judging appearances in mass by means of results in figures.

When statistical methods are to be used for strictly scientific purposes the procedure must be systematically correct. This system can be illustrated by some of its rules.

The first rule of a statistical investigation is: the phenomenon to be counted must be a countable fact that can serve as a unit. The fact must be clearly and definitely separable from all other facts.

In many cases it is a very simple matter to say what things are countable; in others it is very difficult. At first sight it would seem easy to count the number of houses in a town. But how large must a building be in order to be a house? Is every place inhabited by human beings a house? In a recent census of houses in India the very greatest difficulty was found in deciding where to draw the line.

A non-countable object can often be transformed into a countable one by introducing some qualification. We cannot count the number of hot days in a year, because there is no definite limit between hot days and the days that are not hot. We should be continually puzzled as to whether a certain day is warm enough to be counted with the hot days or not, or whether a day that was warm in the morning but cool in the afternoon should be considered a hot day or not. The phenomenon to be counted is not defined in such a way that counting is possible. We cannot count the number of quick children in the schoolroom, because we have no distinct limit between quickness and slowness.

By introducing sufficient limitations the phenomena can often be rendered countable. We can readily count the number of days in the year in which the heat has exceeded 70° F. or the number of children who react to a signal in less than 0.200 s. Yet in a case where such a possibility of measurement exists we are really throwing away an accurate method of observation for a poorer one. If we are to record the temperature of each day, it is evident that some better method of discussing the results could be found than that of choosing some arbitrary figure for a classification of the results. Consider how little we have gained by counting the number of days above 70°. Many of these days might have been terribly hot, or the whole lot might have been moderate. The days below 70° cannot be all cold days; they might have been very cold days, or they might all have been moderately warm. Again, the choice of the limiting temperature may or may not entirely alter the result. If the limit had been put at 75° the number of days above that degree might be greatly changed or it might be almost the same. It is the custom for some health resorts to advertise so many

sunshiny days in the year. How many hours of sunshine are necessary for a sunshiny day? How much cloudiness is necessary to ruin it? It is very plain that without some definite agreement and measurement the accounts of various observers will differ greatly.

In order to decide whether a day is colder or warmer than a certain temperature, or that a child reacts faster or slower than a certain figure, it is necessary to make a record for each case. But when such records are at hand it requires very little more labour to treat them according to the methods of measurement. By a merely qualitative statement we actually throw away a large portion of the results of our records.

The second rule of a statistical investigation is that all things which are to be counted shall correspond completely and exactly to the stated definition of the counted object and that nothing that does so correspond shall be omitted. This requires that all the properties of the thing counted shall be accurately determined before the count begins and that they shall not be changed during the counting.

For example, if we judge the culture of communities by the number of universities in relation to the population, we find that the single state of Ohio, U.S.A., is about twenty-five times as cultured as Germany, a statement that even a native of Ohio would hardly assert to be true. The trouble lies in the failure to properly define the term university.

Again, statistics of children under the headings "bright, average, dull; bright in general, bright in some particular," &c., convey little meaning owing to the indefiniteness of the terms used. The statement that the errors introduced will compensate themselves when large numbers are taken is not only misleading but also false, as these terms are subject to

definite systematic influences from the particular conditions under which the judgments as to brightness are made. A study of St. Louis children led Porter [1] to the conclusion that tall children are brighter than short children. His measurements of height were according to a well-defined standard, but the judgment of "bright" or "dull" was made on the basis of examination marks. It is hardly needful to remark that the children who receive the best marks are often those of steady character but only moderate ability. It is, moreover, the experience of every teacher that the brightest boys generally receive only moderately good marks. It is also a fact that many of the apparently dull boys, when removed to a congenial atmosphere, turn into successful men. What is true of the more prominent cases will be true of the mass of pupils who are always along the dividing line. Judged by a different standard the bright and the dull would frequently change places.

The inaccuracy in defining "bright" and "dull" children is to a slight extent avoided by taking the teacher's personal judgment instead of school marks. Gilbert [2] tested this definition and found, for the children of New Haven, Conn., apparently a relation between brightness and rapidity of response to a signal, but no relation between "brightness" and other mental qualities. There was no relation whatever between height and "brightness."

The attempt to define "bright children" and "dull children" according to school rank or according to the teacher's impression violates every item of the rule. Some will be counted into one class who ought not to

[1] Porter, *The Growth of St. Louis Children*, "Trans. Acad. Sci., St. Louis," 1894, vi. 335.

[2] Gilbert, *Mental and Physical Development of School Children*, "Stud. Yale Psych. Lab.," 1894, ii. 71.

be in it, and some will be omitted who ought to be in. This is due to a failure in the fundamental requirements of an accurate definition of the terms employed.

After the statistics have been gathered with the requisite care, and have been subjected to the most careful scrutiny, it becomes necessary to attach to the results degrees of validity based solely upon the results themselves. Let me explain briefly by means of a non-psychological example, what is meant by the "degree of validity."

If a cubical die with one black face and five white ones be thrown a great many times, it will be found that one face appears upward about as often as any other, or about $\frac{1}{6}$ of the time, and consequently that a white face appears about 5 times as often as the black one. If we suppose the die to be perfectly homogeneous and all the faces and edges to be just alike, we can well believe that, as the number of throws is increased, the number of appearances of the black face will approach more and more closely to the fraction $\frac{1}{6}$ of the total number of throws. This fraction we call the probability of the appearance of the black face. The total number of faces of which any one may appear is $n=6$; the number of black faces is $r=1$, and of white faces is $s=5$. The probability at any given throw for the appearance of the black face is $p=\frac{r}{n}=\frac{1}{6}$, and against its appearance is $q=\frac{s}{n}=\frac{5}{6}$.

This does not mean that the black face appears exactly once in 6 times. There may be a run of blacks so that for a certain set of 6 throws the black may appear every time. Nevertheless, in the long run the black will appear about $\frac{1}{6}$ of the time, and the longer the run the more closely the fraction will approach $\frac{1}{6}$.

The expression, "about $\frac{1}{6}$ of the time" has been

accurately defined as depending on the number of throws. That is done in the following way:—

Suppose that we establish a scale of certainty extending from 1.000... for positive results down to 0.000... for negative results. If a black face is to be expected with a certainty of 1.000... we know absolutely that it will appear; such would be the case if all the faces were black. If it is to be expected with a certainty of 0.000..., then no black face would appear; such would be the case if all the faces were white. Let us see what certainty we are to attach to the fraction $\frac{1}{6}$ when we say that at any given throw with our original die the odds are 1 to 5 for the black. The solution has been found by Bernoulli. If n throws are to be made with our die, we can expect with a certainty of

$$P = \Phi(\gamma) + \frac{e^{-\gamma^2}}{\sqrt{2\pi n p q}}$$

that the actual result differs from the ideal probability $p = \frac{1}{6}$ by an amount which lies between

$$\pm k = \pm \gamma \sqrt{\frac{2pq}{n}}$$

The expression $\Phi(\gamma)$ is the well-known probability-function. A table of its values will be found as Appendix I. The quantity e is the basis of the natural system of logarithms, or $= 2.71828$.

For example, supposing we are to make 3000 throws with the die, with how much certainty may we expect that the black face will appear not more than 505 times or less than 495 times; or, in other words, that it will not differ from the ideal $\frac{1}{6}$ by more than $\pm\frac{5}{3000} = \frac{1}{600}$? Here we have $n = 3000$, $p = \frac{1}{6}$, $q = \frac{5}{6}$, and $\gamma \sqrt{\frac{2pq}{n}} = \frac{1}{600}$.

Performing the operations indicated,[1] we find that the degree of mathematical certainty is 0.21 that the assigned limits will not be exceeded. If, instead of a range of variation of ± 5 from the ideal 500, we assign successively ± 6, ± 7, ± 8, &c., we get greater degrees of certainty; if we assign successively ± 4, ± 3, ± 2, and ± 1, we get lower degrees. For example, for ± 10, we find $P=0.38$; or the chances are not quite 4 out of 10 that the limits will not be exceeded.

By similar methods we can tell just what limits of variation to assign for any desired degree of certainty. In statistical work we generally demand a degree of certainty of $P=0.9999778$, which is so near the unit mark as to be reasonably reliable. For this value[2] $\gamma=3$. In the case just treated the allowable limits of variation from 500 for practical certainty can be stated as ± 86.

Let us now take a psychological example. An investigator publishes a series of conclusions in regard to the overestimation of an interval of time when filled with one kind of visual impression as compared with an interval filled with another kind. A slowly rotating cylinder presents colours before a square opening in a screen. The opening appears of each given colour for a definite length of time, and the observer states whether the two intervals of time appear equal or not. In one set of experiments a time interval filled with a constant colour is compared with another filled by a many-coloured band. In a total of 600 experiments

[1] The scheme for solving such problems is given, *e.g.*, in Meyer, "Vorlesungen über Wahrscheinlichkeitsrechnung," 107, Leipzig, 1878, and is reproduced with slight changes in Appendix II.

[2] For practical work it is sufficient to use the formula $k=3\sqrt{\frac{2pq}{n}}$. The result is a trifle too large, but the error is on the safe side.

made on six persons the constant colour appeared of longer duration than the changing one in 58.7% of the cases. The conclusion is drawn that the constant colour actually seemed of longer duration.

What degree of validity is to be given to this statement? When it is required to say that one of two things is longer than the other, mere guess-work without any knowledge would make one of them longer in 50% of the cases. Here we have 58.7% for one of the things. Making the supposition that absolutely no errors were made in setting up the problem and carrying out the experiments, we find that according to the theorem of Bernoulli $P=0.24$, or that the degree of validity is about $\frac{1}{4}$ on the scale of certainty which we have assumed. Of course, for a less stringent scale the degree would be greater.

Up to this point we have treated of mathematical probabilities; we have assumed an ideally perfect die, or we have supposed the statistics to be absolutely free from error. To such cases we can apply the laws of chance as given in mathematical works on probability. In actual investigations, however, it must first be proven that the results follow the laws of probability.

Suppose we have as the result of a statistical investigation the fraction $\frac{m'}{n'}$, where n' is the total number of cases and m' the number that have a certain character; can we assume that the co-efficient of frequency $p'=\frac{m'}{n'}$ can be treated as a mathematical probability p? For example, in the case of time-estimate just considered $n'\pm 600$, $m'=352$ and $p'=0.587$; we have treated p' as a mathematical probability, and we must ask if this treatment was justifiable.

If p' is a mathematical probability, a repetition of

the statistical count will give a value p'' which does not differ from p' by more than

$$l=\gamma\sqrt{\frac{2p'q'}{n'}+\frac{2p''q''}{n''}}$$

where $p'=\frac{m'}{n'}$, $q'=\frac{n'-m'}{n'}$, $p''=\frac{m''}{n''}$, $q''=\frac{n''-m''}{n''}$ and γ is the quantity already used in considering Bernoulli's theorem. The amount of difference depends on the degree of validity we desire. For the usual statistical validity we take $P=\Phi(\gamma)=0.9999778$, whereby $\gamma=3$. If, in a statistical investigation where n' and n'' are large, $p'-p''$ exceeds $\pm l$, it is safe to assume that the two cases follow different laws of frequency. If $p'-p''$ exceeds $\pm l$, they may follow the same law of frequency, although there is no proof that they actually do so.

In the time-experiments just mentioned the experimenter took the average of the percentages for six observers; he assumes thereby that the results of each observer follow the same mathematical laws of probability. Let us take the first two observers each with 100 experiments: B, 59.5% and H, 63.8%, with a difference of 4.3%. Does this difference exceed the limits allowable for considering them as following the same laws of probability? Here $p'=0.595$, $q'=0.405$, $n'=100$, $p''=0.638$, $q''=0.362$, $n''=100$, $\gamma=3$. We can in a case of this kind consider 100 as a large number. We find that the allowable difference is 0.282, or 28.2 %, and consequently we can assert that the laws of frequency in the two cases may possibly be the same.

Even when a series of counts does show an agreement sufficient to forbid our concluding from the above formula that the laws of frequency are different, we have not yet proven that they are the same. To prove this, it is necessary to show that, as the number of

experiments or counts increases, the general value p' converges toward a limit p in the fashion required by the laws of probability.

Suppose that n counts of z items each, n being large, give successively the co-efficients of frequency, $p_1, p_2, \ldots, p_n$. If these are mathematical probabilities, the most probable value for p is $a = \frac{p_1 + p_2 + \ldots + p_n}{n}$. Moreover the variations of $p_1, p_2, \ldots, p_n$ from a should be grouped on either side of a according to certain laws. Even with only a fair number of series we can expect that the numerical values (regardless of sign) of p will be about half of them greater and half less than the probable error, $r = \frac{0.4769\ \sqrt{2\,a\,(1-a)}}{\sqrt{z}}$. If the counts and series could be infinite in number we would get a "true" value W for the result; in a limited series we are confined to the most probable value a. The trustworthiness or "precision" of a in representing W is indicated by the value of r; that is, if two independent series are carried out under like conditions, we can trust them inversely as the relative values of r.

Each value of $p_1, p_2, \ldots, p_n$ has likewise its precision, namely, $h = \frac{0.4769}{r} = \frac{\sqrt{z}}{\sqrt{2\,a\,(1-a)}}$. If we take the single variations $x_1 = p_1 - a$, $x_2 = p_2 - a, \ldots, x_n = p_n - a$ and put $\gamma = h\,x$, we have, with γ in its usual meaning, an expression that indicates the probable frequency with which the errors are to be found lying within $\pm x$. The variations should follow a certain law of frequency, as indicated by the successive values of $\Phi(\gamma)$ in the Table for γ. For practical work it is generally sufficient ($P = \Phi(\gamma) = 0.50$) to assume that about half the errors lie within and half without $\pm d = \pm \frac{0.4769}{h}$. If they do we

conclude that the statistical results may be treated as mathematical probabilities; and if they do not, that they may not be so treated. If more than half fall within $\pm d$, the separate experiments or counts were probably not independent of each other but causally connected; *e.g.*, each successive experiment might by practice make the observer more skilful, and the results would therefore hold good neither for the unpractised nor for the practised observer. If more than half fall without $\pm d$, we must conclude that uncertain influences are at work, *e.g.*, fatigue, distraction, &c.[1]

Let us see how these values of probability are to be applied to the results of a study of memory. We now take a published report of an admirably executed investigation, and proceed to apply the rules with no knowledge of what the result may be, and with no anticipation excepting a favourable impression from a cursory glance at the figures.

For visual memory the apparatus consisted of several series of ten small squares of paper of different colours, and of several series of black numbers each mounted on a square white card of the same size as the coloured squares; they were exposed on a black background. For auditory memory the names of the colours or the numbers were spoken by the conductor of the work. Several corresponding series of coloured squares and mounted numbers were supplied to the persons experimented upon. After having seen the original series, the subject arranged these latter colours and numbers in the order of the original series as well as he could remember. The subject saw or heard the original

[1] For the brief view of statistics which I have presented I am much indebted to Lexis, "Einleitung in die Theorie der Bevolkerungsstatistik," Strassburg, 1875; "Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft," Freiburg, i. B., 1877

series only once ; the recollecting was done immediately at the conclusion of each series. Thirty-two different series with a total of 2,140 colours and numbers were used.

Proceeding to the results, we will assume that the experiments are entirely free from sources of error, and that the results can be accepted just as they are stated.

The result for the first person experimented upon for visual memory is 420 errors out of 2,140 colours and numbers, or 18.7%. The result is a single fact in itself, but before we can use it we must prove that it is a true probability. The author should have done this by comparing the results of the separate series that go to make up the 2,140 results, but, as this was not done, the only light we can get on the subject is by comparing together the results for the five subjects. These results are 18.7%, 19.3%, 17.1%, 22.4%, 25.1%. If the results are governed by the laws of probability, these numbers should not exceed certain limits. Let us take the extreme cases 17.1% and 25.1% ; if these do not exceed the allowable limits, the others, of course, do not. Putting, as previously explained, $p'=0.171$, $p''=0.251$, and $\gamma=3$, we find that the allowable difference is $l=0.053$, whereas the actual difference is 0.080. We are apparently justified in assuming with practical certainty that the two cases follow different laws, and that we cannot apply the deductions of probability.

But what difference does it make, however, whether they follow the laws of probability or not? Simply this ; no special laws were established for these results, therefore we try to apply at least the law of chance. If they do not follow even the law of chance, our work stops completely ; the figures may be of interest as personal facts, but if these facts are utterly capricious, we cannot use them for scientific deductions.

Let us remember, however, that these were the extreme cases, and that the disagreement between the calculated and the actual difference is not so very great. We can, without very great wrong, give up our deduction of disagreement and leave the question open for a positive answer. Let us assume, then, that the results have stood the first test and proceed to the next test.

The most probable value for a series of results is assumed to be the average. The average for the five subjects is 20.5 %; are the variations grouped as demanded by the laws of chance? We find that the number of counts (unfortunately not large) is $n=5$ and that the number of items in each count is $z=2140$. The most probable value for the five percentages is

$$a=\frac{0.187+0.193+0.171+0.227+0.251}{5}=0.205$$

The differences between a and the individual results are successively 0.018, 0.012, 0.034, 0.019, 0.046. Two or three of them should be greater and two or three less than $r=0.006$. As they are all greater, there is no proof that they follow the same laws of probability. We cannot apply the calculations to the separate values for the single observers, as no data are furnished by the authors.

Since there is no evidence whatever that the laws of probability govern the case in hand, we cannot use the average for the five subjects. For, if the average is assumed as the most probable value the laws of probability should follow the deduction, whereas they do not; on the other hand, if the average is assumed as a capricious value, it has no scientific worth. We can, therefore, consider only the results of single subjects. All data fail that can enable us to judge whether the laws of probability or any other laws hold good or not. If not, our work stops completely; as, however, the

authors proceed to draw deductions, we will assume, in order to keep the illustration, that the laws of probability are valid for the particular cases, and will proceed to test the conclusions.

We will use the results for the first subject mentioned. The number of errors for visual memory was 18.7 %; for audible memory, 34.6 %; for visual and auditory working together, 44.4 %. The conclusion is drawn: when the two senses act together in recollection, they hinder each other. This result is so unexpected that we must ask whether this difference is great enough to warrant the conclusion that pure chance may not have produced it. Proceeding as before, we find that we can say that, with practical certainty, the difference 0.444 — 0.341 = 0.103 exceeds the limit 0.053 allowable for considering them as following the same law and arising from chance. In other words, the difference is a real one. The same holds good for each of the other particular cases, and, admitting the assumptions we have made, we can decide that the general conclusion is valid for all the cases.[1]

[1] To serve as a subject for dissection is a thankless task. In looking over a number of statistical investigations for this purpose, I finally decided to use the best piece of work I found. The particular investigation selected thus pays the penalty of its goodness.

CHAPTER III.

MEASUREMENT.

WHENEVER we are treating of an event that either happens or does not happen, we are working with qualitative facts; but whenever one thing may be greater or less than another, the phenomenon observed has a quantitative character, and can, if proper instruments are provided, be made the subject of measurement. Phenomena upon which we can pass judgments of greater, equal or less in relation to one another, we call *quantities.*

If a quantity be expressed by a concrete number, *i.e.*, by an expression composed of a number and the name of the unit-phenomenon, we call the result the *value* of the quantity; the process whereby the value is obtained, we call *measurement.*

We are all familiar with commercial and physical measurements: we have all measured butter or cloth, temperature or electric potential. But are there such things as "mental" measurements? Can we measure our sensitiveness, our judgment, our will power, our memory, our feelings, our beliefs? Many of these can be measured without difficulty, others only roughly, still others not at all. For none of them, however, is measurement impossible; the same fundamental method is applicable in all cases, and if it has not been applied

to all mental phenomena, the fault lies in the lack of men of ability to devise the means of applying it.

The first step in establishing any fact as a quantity consists in determining that it is equal or unequal to, greater or less than, another fact.

What are meant by "equal," "unequal," "greater," and "less"?

Psychologically, that is, from direct experience, we all know without more ado. If in each hand we lift a glass of water, we can say whether we can distinguish a difference in heaviness or not. If we can distinguish a difference, we say that they are unequally heavy; generally we can also say that one of them is more or less heavy than the other. If we distinguish no difference, we say that they are equally heavy.

But are they "really" equal? Is there not a physical method of determining their "real" equality as contrasted with "apparent" psychological equality?

Let us make the two glasses of water appear equal in heaviness, so that no difference is distinguished between the glass in the left hand and the glass in the right hand. There is a possibility that these two glasses, judged to be equal when glass *A* was lifted by the left hand and glass *B* was lifted by the right hand, may not retain their apparent equality if the circumstances are reversed. Let us try them with glass *B* in the left hand and glass *A* in the right hand. They appear, let us say, no longer equal; *B* is heavier than *A*. To make them alike we must add water to *A*.

If we do make them alike in this way, *A* will be heavier than *B* when we return to the original arrangement. Evidently, in order to make them equal, we ought to add to *A* just half as much water as the difference between the two, and to take away a like amount from *B*.

Suppose we make A on the right hand equal to B on the left hand by adding a quantity of water to A; then, by pouring a like quantity off, let us get the difference separately. This difference D we divide in two by pouring it into two glasses and adjusting the two portions till they appear equally heavy, one of them, A, being on the left hand, and the other, B, on the right hand. We want to get $\frac{D}{2}$; we actually do get $D = a + b$. On changing the glasses to opposite hands we again find a difference. It is evident that a division of the first difference into the two parts, a and b, was not into halves; we did not get $\frac{D}{2} = a = b$. To correct a and b so that the two parts shall be equal, we evidently need to add half the difference $d = a - b$ to the lighter half. We must proceed to divide this new difference in two as before, but again we fail to get exact division.

By continuing in this way we finally come to a quantity so small, that we can detect no difference between its two halves. We can then add all these halved difference to A, and can say that A is equal to B, at least, as nearly as we can judge by lifting with two hands, thus,

$$B = A + \frac{D}{2} + \frac{d}{2} + \frac{d}{2} + \frac{d}{2} + \frac{d}{2} + \text{etc.}$$

Psychologically they are equal; are they "really" equal? Let us go further by a finer method. If we adopt the method of lifting the two glasses in succession at an interval of about two seconds, by the same hand, we find that we can still detect a difference by this method.

In judging by the one-hand method we lift one glass after the other. This difference in time may, and in

fact does, have an influence. If A is lifted first, and B is adjusted to equality with A, then a reversal of the order by lifting B first will make A appear too heavy. We must therefore proceed in the same way as before to adjust the successive differences, till a last difference is divided in two parts without our being able to distinguish between the parts.

When the two glasses are adjusted by this method till no difference is distinguished, we feel a trifle more satisfied with the equality. The two glasses may not feel equal on any one occasion, yet we recognise that each particular method of judgment differs from every other one, and that no single one gives a perfectly satisfactory equality. As the final equality, we take the result obtained by eliminating the differences due to particular methods. Is the result yet a "real" equality?

When an equality, established in a certain department of mental life, does not give satisfactorily accurate results, we transfer the establishment of the equality to another department. The equality which we have established in our glasses would not be satisfactory for judging gold or diamonds. Let us transfer the judgment to the department of sight. This transference is obtained by means of apparatus.

The usual method in which judgments of heaviness can be changed to judgments by sight is by use of the balance.

When two objects are laid in the pans of a balance the beam tips down. If, when the objects are reversed in the balance, the same one again goes down, it is heavier than the other. We proceed to increase the lighter weight till the beam ceases to tip. The weights are now judged indirectly by sight to be equal. By reversing the position of the weights we see that the beam tips, and consequently that the balance is a little

one-sided. Our judgment of the tipping is made by sensations of sight, and is a mental one. We might proceed to get nearer a sight-equality by halving the differences, as we did originally with the two glasses of water. But we have already learned that the quickest way is to improve our apparatus; so we scrape a little off one of the scale pans, or add a little weight to the other one till equality appears. Is this the "real" equality? No; for by transferring the judgment to a still finer balance a difference is still detected. Thus continuing to use the sense of sight the methods can be made ever finer by means of improved apparatus, but there never comes a time when we can say that we have found a perfectly true equality with absolutely no difference. Our last judgment always ends in a comparison of two sensations, with the knowledge that the judgment can be improved by the use of finer methods. Our ultimate criterion in any case is therefore a psychological judgment of equality. What, then, is the "real" equality? Practically, it is the psychological equality at the last stage to which we have carried it; theoretically, it is the equality at which we would arrive by forever improving our methods of judgment. We have not left the region of psychology for a moment; we improve our arm-apparatus by judging in sequence instead of simultaneously, we use the balance for transferring the judgment to the sense of sight, and we improve the balance for making the sight-judgments finer.

The balance did not take us any more or any less out of the region of psychology than did the arm; the measurement was as much psychological and as much physical in one case as the other, and the "real" equality is likewise just as mental as our original judgments.

Just what is meant by the common term "physical

equality" as opposed to "psychological equality," can be stated as follows. In the physical sciences and in psychology phenomena can be considered as equal when the differences that exist between them are proven to be so small as to be entirely negligible for the purpose in view. The equality is obtained in any way and by any kind of judgment that may be convenient or possible.

This equality is often called "physical equality" to distinguish it from the equality obtained by any particular sense. The real difference in the ordinary use of the terms appears to be this: two things are said to be "psychologically" equal when they are judged directly by the same sense to which the equality refers; and they are said to be "physically" equal when they are judged equal for one sense by means of apparatus involving a different sense, or the same sense under different conditions. When equality is judged by means of apparatus instead of direct sensations of the property involved, it might be better to speak of *instrumental* rather than physical equality.

This instrumental equality may be established between the most different departments. With the scale and the balance we judge weights by sight. With the micrometer caliper we measure lengths that might be judged directly by sight or by touch on the skin or by muscular sensations. This instrument is so arranged that by a peculiar sensation of pressure we can judge differences far finer than otherwise, and the actual judgment of equality is made by this sensation, while the reading of the scale is done by sight. Similar cases are found in all apparatus.

The final reading of the instrument is in all cases necessarily a direct, psychological comparison; this comparison is nearly always by sight. If we read a

thermometer, we compare by sight the top of the mercury column with some mark on the scale. Clock and galvanometer readings and nearly all judgments of length are of the same kind. All measurements, physical as well as psychological, thus consist ultimately in the comparison between two phenomena of consciousness, generally two sensations of sight. All physical measures have been developed out of these psychological estimates.[1] We measure temperature by noting the agreement of the length of the column of mercury with a certain portion of the scale; we measure the strength of an electric current by noting the angle through which the mirror is deflected or through which the needle passes, and this very angle in turn is measured by some length of arc, chord, or tangent; we measure time by the agreement of the angle over which the hand has passed with a mark denoting the end of another angle taken as a standard.

We have thus completed the first step in establishing quantitive relations between phenomena; we have obtained what we consider to be trustworthy judgments of equality. The next requisite in measurement is the establishment of a graded scale of quantities with any one of which the given quantity can be compared in respect to its equality.

In just the same manner as that in which we established an equality of two weights on the basis of a judgment of "equal" and "unequal," we might proceed to establish a whole series of weights, of half, quarter, double, triple, &c. the weight of some standard from which we start.

We proceed on the assumption that $1 + 1 = 2$, and that the equality is to be established by methods

[1] Wundt, *Die Messung psychischer Vorgänge*, "Essays," 158, Leipzig, 1885.

sufficiently fine for the practical purpose in hand. We make two weights equal (physically or psychologically, as you may please to call it) to each other; this gives us 1 and 1. Then we make another equal to both together; this gives us 2. And so on, with $2 + 1 = 3$, $3 + 1 = 4$, &c., till the whole scale is established. In similar fashion we obtain scales of length, time, &c.

This does not imply that, when we take our scale back to the original kind of judgment from which we started, we shall find it to be confirmed. We did not even find the equality confirmed. It was a peculiarity of the judgment by simultaneous lifting that one hand overestimated the weight as compared with the other hand. It may just as well be that the scale of apparent multiples may be quite different from the one just established. If out of a series of weights exactly alike in appearance, we pick out, by lifting, the set that apparently bear the relations of 1, 2, 3, 4, &c., we find that we have chosen those that do not agree with the judgments of $1 + 1 = 2$, $1 + 2 = 3$, &c. We are mentally so constituted or mentally so trained that apparent judgments of a succession of 1, 2, 3 may not agree with the scale. These differences represent peculiarities in the construction of the individual at the time.

Some confusion has been caused by the statement that psychologically we are able to judge only equality and inequality, with implication that in physics we are able to do something more, namely, to measure one object as a fraction or a multiple of another. Even if in physics we could directly do this, it would mean nothing more than that we could do so in psychology also, because when we compare two physical lengths we are only performing a psychological process. When we say that one line is apparently three times as long as another, we simply mean that the two mental pictures

bear that relation, or that the series of muscular sensations produced by running the eye over the lines bear that relation. The fact is, however, that in the absence of graduated scales we express one quantity as a multiple of another only by estimates from our sensations. The graduated scales, by means of which we always work wherever possible, and by means of which we obtain the accuracy of modern science, are really only records of direct judgments of likeness or difference. The zero point on the thermometer means that the mercury column occupied that place under certain definite conditions, namely, immersion in the water of melting ice; the 100° mark means that the column was just so long when the thermometer was surrounded by steam at 760 mm. barometric pressure. We usually divide the intermediate space into 100 parts, but these divisions of themselves mean nothing. It is only by placing the thermometer in liquids of the intervening temperatures and directly recording the height of the column at each temperature, that we get a definite graduation. As this latter method is too cumbersome, the marks are made at intervals by the dividing machine and then the actual value of each mark is determined by sending the mercury up to it, and noting the temperature required to do so. Thus each mark on the thermometer means that at some previous occasion of a certain character the mercury column reached to that point; when we now make a measurement of temperature we simply compare the length of the column at present with the record of its length for some previous time. The same is true of the galvanometer, the clock, and all apparatus in which the graduation is in units of length; exactly similar processes are used to arrive at other scales.[1]

[1] Scripture, *Psychological Measurements*, "Philos. Rev.," 1893, ii. 678.

Having established such scales of weight, length, time, &c., what do we measure with them?

In the first place we measure our sensations.

In using the word "sensation" I am not introducing any of the technical terms usually employed in psychology. We here have nothing to do with the usual distinction between "sensations" as elements of mind, "percepts" as compounds, &c. In practice this distinction is not carried out. The subject of colour is treated under the heading of sensation, whereas much of the colour work deals with highly compound mental facts. Again, under the heading "perception" you will find, for example, the whole treatment of space, whereas the elements of space are as simple as anything in mental life. With terms such as sensation, perception, intellection, emotion, conation, &c., we have nothing to do; we shall find all the facts of mental life in their proper places, and—I venture to hope—in connections more natural and intelligible than when arranged grouped to suit a particular scheme of classification. Therefore, when I use a word like sensation, feeling, emotion, &c., I do so only in the meanings implied in common speech. Everybody knows what a sensation of warmth or a feeling of hunger is, but there is no need of an attempt to prove that the sensation of warmth is an elementary process, and that hunger is not an emotion. We might possibly speak of measuring sights, hearings, touches, &c., but such expressions are contrary to the genius of our language, and we are accustomed to employ such terms as "sensations of sight" for sights, "sensations of sound" for hearings, &c.

We measure, then, our sensations of sight, hearing, touch, warmth, &c. The millimetre measure that lies before me is one of my sensations of sight; I know it to be a scale established in a way similar to that in

which the scale of weight was established. Here is a line drawn on a piece of paper. This line is also one of my sensations. Applying the scale to the line, I find an equality between my line and a certain mark on the scale, and the value thereby deduced is the measure of the length of my line. I have measured the length of one of my sensations of sight. It does not make a particle of difference whether there is an extra-mental world with an extra-mental scale and an extra-mental line; both the scale and the line are facts of my conscious experience, and I measure one by the other. It would be the same if the line were what is called a "hallucination"; I use the scale and I measure it regardless of whether I have reason to believe it to be a "real" or an "unreal" line. In an exactly similar manner we apply measurement to the other regions of mental life.

Again, we measure the accuracy of judgment. In all the methods of lifting weights we find "sources of error." The difference due to using two hands can be called the error of the two-hand method; its size can be roughly determined after the equality-adjustment by the same method, or it can be more accurately determined after the equality-adjustment by the balance. The difference due to using one hand can be called the error of sequence, the second weight appearing the heavier.

We might correct the error of the two-hand method by placing an extra weight on one hand, the size of this weight being found by the two-hand method itself. In a similar manner we might correct the error of the one-hand method by always using an extra weight for the first one to be lifted. We correct a balance in the same way by adjusting the scale pans till the beam remains steady when the weights are reversed. With

cheaper balances we scrape the heavier horn pan or we put bits of paper on the lighter pan. We thus measure the errors for the particular methods.

I have spoken of "sources of error" in these different methods of measuring. They are "errors" only from the point of view of trying to reach absolute equality; otherwise they are phenomena for investigation. The error of the balance, like all other errors of apparatus, is a property of the apparatus. This latter, however, very properly bears the name of "error of the apparatus" because the balance was built for the particular purpose of establishing equality, and all deviations are opposed to its purpose. Nevertheless this very error is in certain cases made the subject of investigation. The "error" of the two-hand method is a property of the person lifting the weights, and, unless we are trying to compensate or eliminate it, we would no more call it an "error" than we would call anger or an association of ideas an error. It is itself a mental quantity and should also be measured.

When trying to eliminate it, we may call it the "error of judgment," otherwise it is better to name it the "inaccuracy of judgment." Here, again, I use a term in the meaning given to it by everybody. Speculate as much as you please about the processes of logical thought involved; but when I lift two weights and say "unequal," I know nothing of such processes. I have a very definite feeling that I express by saying "unequal," and it is this feeling that I term judgment. The expression for this feeling is found, for this particular case, in a certain difference between weights. Inaccuracy of judgment is the term applied to this difference.

We not only measure sensations and the inaccuracy of judgment, we also measure sensitiveness, or fineness of judgment.

Let us use a set of fine weights on a rough platform scale. We find that the scale fails to notice a difference of less than 60 g. on a weight of 120 g. On using a very ordinary grocer's balance we find that the balance notices a difference of, say, 6 g. on 120 g. The sensitiveness of balances or scales is said to be inversely proportional to the just noticeable differences in a given weight, or directly proportional to the recipocals of these differences. Thus, for the weight of 120 g., the relation of the sensitiveness A of the platform scale to that B of the grocer's balance is given by $A : B = 6 : 60 = \frac{1}{10} : \frac{1}{6}$. Or, the balance is ten times as sensitive as the platform scale.

By lifting the weights with one hand or two hands we can evidently measure our own sensitiveness in exactly the same way as we measured the sensitiveness of the balance.

We get for one person with the two-hand method a just noticeable difference of p_1, and with the one-hand method p_2; for another person q_1, q_2, &c.

The sensitiveness in each case is said to be in the ratios of $\frac{1}{p}$, $\frac{1}{p^2}$, $\frac{1}{q}$, $\frac{1}{q_2}$, &c.

This sensitiveness is generally called the "sensitiveness to differences."

What is this "sensitiveness"? This term is used to-day throughout experimental psychology, much as the word "force" was lately used in physics to indicate a vague, mysterious agency governing phenomena. Scientific thought demands precision of the concept. In the first place, to speak of "sensitiveness" as determining the just noticeable difference seems wrong. All we know about it is the just noticeable difference; sensitiveness is a term by which we compare objects in inverse order to the just noticeable differences.

With the various scales of units thus established, it might seem that nothing more is necessary than merely to apply them. This is not the case. The application of scales to quantities to be measured is not a simple process to be performed by everybody; there is a refined and difficult art of doing so and a well-developed science of treating the results.[1]

The fundamental principles of the science of measurement are somewhat as follows :—

In general it is not sufficient to measure a thing only once; we repeat the measurement a number of times. If the measurements agree, we are justified in concluding that our apparatus is not fine enough to detect the differences due to the infinite number of sources of error always present. By using finer apparatus the accuracy of the result can always be pushed one or two decimal places beyond agreement. For example, in measuring the sensitiveness of a balance, if we always obtain the same result, we may feel sure that the weights we have used for the measurement were not finely enough graded. We know that from changes in the position of the knife-edges, from friction, from temperature, from air currents, &c., the results of our measurements will vary, provided our weights be fine enough. The like holds true for all measurements, psychological as well as physical.

Suppose I attempt to measure the diameter of a coin. I report that in ten measurements the result was $\frac{7}{8}$ of an inch every time. I now take a fine steel mechanic's ruler, graduated in 64ths of an inch, and again measure the coin ten times. I report the diameter to be $\frac{57}{64}$ of an inch on nine occasions, but $\frac{56}{64}$ on one occasion. From previous experience you know that my eyes are sharp enough to

[1] Scripture, *Accurate Work in Psychology*, "American Journal of Psychology," 1894, vi. 427.

use the ruler correctly, and you suspect that the source of disagreement lies in the coin. You therefore limit me to one particular diameter between two finely marked points on the circumference. In placing these points you may not have got them on a line exactly through the centre ; but we will overlook this error and consider the linear distance between these two points to be the true diameter of the coin. To free myself from all responsibility for your assumption that this distance is the diameter of the coin, I state my problem anew as the determination of the linear distance between two marks on the edge of the coin.

Using the mechanic's ruler, I always obtain $\frac{57}{64}$ as a result. Your previous experience tells you that agreement means comparatively coarse measurements ; you advise a scale graduated in tooths of an inch and the use of a magnifying glass. I obtain 0.89 of an inch without disagreement. This work is very trying to the eyes, and if I keep on making measurements they begin to disagree owing to fatigue. I find that I cannot get the divisions of the ruler exactly over the marks. The subdivision of the ruler might be pushed further, but nothing would be gained, as the inaccuracy of the eye would produce disagreement. You therefore suggest the transference of the measurement to the sense of touch.

Therefore I now apply a micrometer caliper to the coin. I screw up the head till the rod just touches the coin. What do I mean by "just touches"? I mean a slight but certain sensation of resistance. I can make the sensation weaker or stronger, whereby the rod presses against the coin less or more; the instrument yields, and I must settle on the degree of yielding to be chosen. Long practice has made me use a certain degree which I keep in memory.

When the ruler was used, we assumed that it was correctly graduated ; likewise we will assume that the graduations on the rod and the barrel of the caliper are correct. I report to you that the measurements no longer agree. You reply that my hand must be out of practice. I practice for a while measuring a piece of steel till I get no disagreement when measuring. I try the coin and still find some disagreement. My method is fine enough to be blameless and you suggest that, owing to the wideness of the marks on the edge, I do not always get exactly the same line. I now fix the coin and one end of the caliper immovably together. Agreement results, and I report the diameter as 0.887 of an inch.

Agreement always means that the method should be made finer. On the back of the caliper I find a graduation enabling me to read to the 10,000th of an inch. First proving to your satisfaction that my hand is delicate enough for adjustment to the 10,000th of an inch, I report that there is now disagreement and that the average gives 0.8869 of an inch. And so we might go on.

If the measurements agree, the conclusion is that the method of measurement is not fine enough to detect the differences due to indefiniteness in stating exactly what is to be measured, or due to changes in the quantity measured owing to neglect of conditions that should be kept constant. If the measurements disagree, the conclusion is one or both or all of three things :—(1) The method of measurement is inaccurate to a certain amount ; (2) the irregularity is due to indefiniteness of the problem ; or (3) changes occur in the quantity measured.

In the case of agreement we can go no further without finding new methods and apparatus. In the

case of disagreement we can, by making the method more accurate, by more carefully defining the problem, or by more accurately controlling the quantity measured, study any one of these three facts. Illustrations of how this is done will be found in Part II.

How shall we treat our results where they disagree? Suppose that we have made a set of measurements to such a degree of fineness that the last few decimal places disagree. In stating the result of our work we cannot give every individual measurement; some one value must be chosen or deduced which best represents the lot. The most frequent representative is either the average or the median. Such a representative result is generally called a mean.

The average is found by adding together all the results and dividing by the number of results. Suppose we had a series of nine measurements of the time it takes to walk from our front door to our office, say 3041, 3047, 3039, 3042, 3046, 3049, 3045, 3094, 3047 seconds. To find the average time we add them all up and divide by nine. To make matters perfectly general, let us denote the number of seconds in the first result by a_1, in the second result by a_2, &c., up to a_n. Then we have for the average $a=\frac{a_1+a_2+\ldots+a_n}{n}$. It is evident that this formula will serve for bushels of wheat, dollars, years, or anything else that can be expressed in multiples of a concrete unit.

The median is determined by counting off the results in the order of size and taking the middle one. This can generally be done by simple inspection; for the sake of clearness let us in our examples actually arrange the results in this order. Beginning with the smallest, we have 3039, 3041, 3042, 3045, 3046, 3047, 3047, 3049, 3094. The middle one or median is 3046.

I cannot here go into a discussion of the relative advantages of the average or the median.[1] I will only point out that an extreme result, like 3094, exercises a far greater influence on the average than any one of the others does, whereas it does not have any more effect than any other result in determining the median. The average is the best representative value for some purposes; it is the customary one for all. In psychological work I have found that the average frequently misrepresents the group of results, whereas the median does not. In those cases where the average gives a good result, it turns out to be practically the same as the median. In this book I shall use the average almost exclusively in order not to introduce what might seem a strange term.

It is not sufficient to know the mean result (average or median); we must know something of its uncertainty. This is most readily determined by calculating the mean variation.

Let us write the original results in a column, add them up, and find the average. Then we find the difference between each result and the average (never mind the

a	v
3041	9
3047	3
3039	11
3042	8
3046	4
3049	1
3045	5
3094	44
3047	3
9)27450	9)88
3050	9.9

[1] See Appendix III. Those especially interested will find the subject treated in "Studies from the Yale Psychological Laboratory," vol. ii., New Haven, 1894.

sign), and write it in the column *v*. Now we average all the figures in this column. The result is called the mean error or mean variation. Here we have a measure of uncertainty.[1]

The mean error for the median is found in exactly the same way. The small numbers here indicate a con-

m	*v*
3041[2]	5
3047	1
3039[1]	7
3042[3]	4
3046[5]	0
3049	3
3045[4]	1
3094	48
3047	1
3046	9)70
	7.8

venient way of checking off the results to find the median.

The mean error is often the only means we have of judging the reliability of a representative result. If the methods of measurement have been careless, the mean error will be large.

The publication of averages or medians without a statement of the mean errors can happen only from two causes: 1. Unintelligibility to the reader, as in elementary and popular books. 2. Stringent need for condensation.

Up to this point our explanations apparently apply to all measurements, physical and psychological; is there no difference between the two kinds?

Since we always measure physical quantities by

[1] Particulars concerning the mean error and other measures of uncertainty are to be found in works on measurement.

means of a psychological judgment as to the agreement of two sensations or sets of sensations, we must in physical measurements so arrange matters that the psychological judgment introduces only a small uncertainty into the records. Likewise in making psychological measurements, we must so arrange matters that the physical manipulations are so small as to be negligible. Since all psychological and physical measurements are made by means of apparatus, the error of the apparatus must in both cases be sufficiently small in comparison with the quantity measured.

For example, in measuring the time between two successive culminations of the same star, the uncertainty introduced into the results by the variations of our judgments in the graphic method are too small to be of importance for most physical purposes, the length of the sidereal day being determinable within a mean error of 0.05 seconds, or $\frac{5}{10000}$ of 1 %. In this case the measurement is physical, and the psychological error due to irregularity in reaction time is negligible.

In measuring reaction times an outside limit of error of $\frac{1}{100}$ of a second is beyond the needed accuracy, because various mental processes cause a greater fluctuation; the length of the time measured is seldom less than $\frac{100}{1000}$ of a second; we can thus allow an outside limit of error of 1 %. We can therefore use a fork vibrating 100 times per second, whose accuracy has been determined to within 1 %, that is, one whose vibrations during a second are known to lie within 100 $\pm$1. This is determined by comparison with an accurate clock, regulated ultimately by the transit of a star. Here we have the physical errors practically eliminated and the measurement is psychological.

The accuracy required for astronomical purposes, in the first case, was something far beyond that required

for psychological purposes in the latter case; yet the very thing which we wanted to measure psychologically, the reaction time, is used to determine the unit of astronomical measurement, the second. In the latter case we arranged our experiments so that the variations of the psychological quantity were negligible, in the former so that the inaccuracy of the physical apparatus was negligible.

It is readily seen that if we do not eliminate or render negligible the psychological sources of variation in physical measurements, as was the case in astronomy before the discovery of the personal equation, we are introducing errors into our physical results. Likewise, if we are measuring psychological phenomena, and yet do not know how much of our results and how much of the variations are due to mental influences, and how much to the apparatus, we really do not know what our results mean.

It is from the side of critics who are not acquainted with the science of measurements that we often hear the remark that it is of no use to be exact in psychological work. They are led to such assertions by productions which are defective in their methods of statement. In the published accounts of such work there is often no information as to elimination or presence of errors. An experimental result whose reliability is unknown to us is worthless. In order to form a judgment on the accuracy of the result, all the necessary data must be given. Any description of a method or a result can be criticised as materially incomplete if it does not give all the data needed for such a judgment.

In making psychological measurements it is then our first duty to establish a scale of units whose uncertainty and inaccuracy have been by some method reduced

below the required limit. We must establish our system of time-units, our system of space-units, our system of energy-units, and our various systems of secondary units with the needed accuracy. Then we must arrange and test our apparatus till its errors are too small to be regarded for our purposes. Thereupon we compare our actual judgments with the system of units, and determine the relations between the two.

In all future discussion we shall suppose that this has been done, and that any deviation or uncertainty regarding a result is entirely due to psychological sources. In such a case (where the instrumental errors are entirely negligible) the mean variation will be a psychological quantity. For example, in discussing reaction times measured by our spark method (which is exact beyond all needs), we shall always treat the mean variation as due to purely psychological causes.

We can thus transform a mean variation into an expression of psychological accuracy and regularity. We can do more. By regulating all except one of the sources of psychological variation so carefully that the portion of the mean variation due to them is negligible, we can study the mean variation itself as representing conditions and changes in the psychological factor investigated. One of the examples of such treatment will be found in Chapter XII.

About psychological measurements there is nothing odd or peculiar; there are no distinctly psychological methods of measurement. Although Fechner, Müller, and Wundt give special fundamental methods of psychological measurements, these are, as will be seen in future chapters, simply skilful adaptations of methods common to all the exact sciences. For example, there is neither more nor less justification for introducing a "method" of minimum changes into psychological

measurements of weight-sensitiveness or time-estimate than there is for introducing it into physical measurements of the sensitiveness of a balance or the fineness of a galvanometer. There are peculiarly psychological methods of measurements in the same sense that there are peculiarly physical, peculiarly astronomical, or peculiarly meteorological methods—*i.e.*, they are not peculiar at all except in the details. Of the four psychological methods usually given: (1) minimum changes; (2) middle gradation; (3) right and wrong answers; (4) average errors; the first is a modified method of finding the sensitiveness of any instrument, the second is not a method at all, but the determination of the middle point between two others in a scale, the third is simply a problem from the science of probability, long since solved by mathematicians, and the fourth is a special treatment of mean errors.

In conclusion, I have made clear, I hope, how we establish psychologically a scale of units, how we transfer the scale from one sense to another according to the problems in hand, how we calculate results, and how physical and psychological methods differ only in one or the other class of errors.

CHAPTER IV.

EXPERIMENTING.

ONE of the ways for improving observation is to introduce experiments.

When an investigator is studying a phenomenon he can never gain more than a cursory knowledge if he waits for the events to come to him. In order to obtain more data he must take an active part in bringing the events about. Thunderstorms have been observed and recorded since the beginning of history; but what a storm really was could not be explained until electrical experiments were made. When once the effects of a storm had been observed, and compared with the effect of an electric spark, the inference was plain that the discharge of the machine was simply a storm in miniature. What the observation of a thousand years had left unexplained was understood in the light of a single experiment.

The art of experimenting is a comparatively modern acquisition in science. With the Greeks physics and psychology were on the same level of general description. "The ignorance of the ancients in regard to the art of experiment, or the meagre development which it met with among them, is the reason why their science of physics was so backward. With the introduction of experimenting there arose the active independent in-

vestigation of natural phenomena."[1] A similar remark might be made concerning part of psychology down to a couple of decades ago. The essential advance that psychology has made, in going beyond what could be learned by observation, is due to the discovery of methods of experimenting on mental processes.

So important to the new psychology is the method of experiment that the term "experimental psychology" is frequently used as synonymous for the whole science. Unfortunately the term is so attractive, and the science has so creditable a standing, that it has been applied where it does not belong. It is necessary, therefore, for the reader to understand what an experiment is and what it is not. Until he does understand this he can no more distinguish between psychological science and psychological error, than he could without artistic cultivation have an opinion on the relative merits of a Rubens and a two-penny chromo.

To illustrate the passage from observation to experiment, and to explain what an experiment really is, I will choose the familiar phenomenon of colour-blindness as an example, and will trace the progress step by step.

It had unquestionably often been observed that certain objects were declared by some persons to be of the same colour whereas they appeared of different colour to other persons. This disagreement in regard to colours can be illustrated by statements of a couple of persons who found that their ideas of colour differed from those of most people. In one case reported, the declaration is made :—

"If railings were painted red, I could not distinguish them from the grass. The grass in full verdure appears to me what other

[1] Poggendorf, "Vorlesungen über die Geschichte der Physik," p. 10, Leipzig, 1879.

people call red ; and the fruit on the trees, when red, I cannot distinguish from the leaves unless when I am near it. A cucumber and a boiled lobster I should call the same colour, making allowance for the variety of shades found in both ; and a leek in luxuriance of growth is to me more like a stick of sealing-wax than anything I can compare it with."

In another case the statement is :—

"I cannot perceive a bit of red sealing-wax if thrown down on the grass, nor a piece of scarlet cloth hung upon a hedge, which, I was told, was to be seen a mile off. I once gathered some lichen, as a great curiosity, from the roof of a friend's fishing-house. I thought it was of a bright scarlet, from its seeming to be the same colour as the tiles ; in reality it was a bright green. On another occasion I perceived no difference in the complexion of a foreign lady who had purposely substituted Prussian blue for her rouge."

Rough observations of this kind must have repeatedly been made in previous times. As we have no reason for supposing such differences in colour-vision to have arisen in historic times, the earliest observations must have dated back to the times when man was forming his simplest language. Yet it was not till 1777 that by rough tests some definite information was gained on the subject.

A narrator states concerning one person :—

"I also showed him a great variety of ribbons, the colour of which he sometimes named rightly, and sometimes as differently as possible from the true colours. These experiments were made in the daytime, and in a good light. I asked him whether he imagined it possible for all the various colours he saw to be mere differences of light and shade, and whether he thought they could be various degrees between white and black, and that all colours could be composed of these two mixtures only. With some hesitation he replied, No ; he did imagine there was some other difference."

Here we have the passage from passive general observation to rude tests in which the observer takes an active part, but the additional knowledge gained

thereby is slight. All that is proven by the tests mentioned is, that the difference in the seeing of colours was actually present, and could not have been a mistake of observation.

An advance was made by Seebeck, who gave to each person tested a number of coloured objects mixed up together, and had him select and group together those which seemed of the same colour. The result showed that there were at least three classes of people in regard to colour, and that among the abnormal ones there were various degrees of defect. Although such crude tests open the way for science they cannot be called scientific experiments. From this point a statistical investigation might have been undertaken to determine how many persons fell into each class. Fortunately this was deferred till the phenomenon could be better understood.

What was needed in order to gain more definite knowledge? In the observations and rough tests reported in these early cases, objects that seem different in colour to most persons are confused by others. Can we not introduce some *system* into the random tests, whereby we can determine just what the abnormal eyes see?

As is so often the case, an ingenious hypothesis suggested a method of experiment. The Young-Helmholtz theory of colour-vision asserted the composition of all colours out of various proportions of three fundamental colours—red, green, and violet. This was used to develop a system of testing.[1]

"To explain the abnormal sense of colours by the theory of the normal, we can in advance, conceive various possibilities. Let us

[1] The fruitfulness of the hypothesis was not injured by its possible incorrectness; see the chapter on Colour.

suppose that one of the three fundamental perceptions is wanting, or that one of the primitive colours is absent; it is clear that the whole chromatic system will be upset. It is evident, therefore, that this system must be completely different, according to the absence of one or the other of the three primitive colours. It is virtually just in this way that it has been attempted to explain cases of a strongly marked defect in the chromatic sense, or genuine types of blindness to colour, found in real life. The term *colour-blindness* has been justified by this, as it indicates in each case a genuine blindness to one of the primary colours. In this way, therefore, we distinguish, according to the kinds of element wanting, three classes of blindness—1st, Red-blindness; 2nd, Green-blindness; 3rd, Violet-blindness."[1]

On the basis of these considerations Holmgren proposed a systematic method of testing by means of skeins of worsted. For definite and well-considered reasons two standard test-colours were chosen, light green and light purple (pink).

The person tested had to pick out from a pile of worsteds of various colours all those that were of the same general colour as each test-colour. The examiner explains that "resemblance in every respect is not necessary; that there are no two specimens exactly alike; that the only question is the resemblance of the *colours;* and that, consequently, he must endeavour to find something similar of the same shade, something lighter and darker of the same colour," &c.

From the colours selected to match the standards the examiner draws an inference concerning the colour-vision of the person tested. The test is apparently very simple, and the conclusion to be drawn from various classes of results are clearly stated. Yet the person starting to make such examinations on others soon finds that the results frequently do not fall into

[1] The four quotations in fine print, derived from various sources, are repeated from Jeffries, "Colour-Blindness," Boston, 1879.

the classes provided, that they often depend on the way the task is presented, that sometimes a repetition of the test will reveal an entirely different result, &c. The successful execution of the test depends very largely on the experience and skill of the examiner in drawing inferences from uncertainty or variation in the manner of handling the wools by the person tested. Even the most skilful railway examiner hesitates in many cases before refusing a certificate of colour-competency to the candidate. No one is justified in claiming that the test is sufficient to detect all the dangerous cases of colour-blindness, for it is a fact that many men do pass the tests unsuspected, who are nevertheless incompetent to distinguish signals with certainty.[1]

Holmgren's test, however, is a true experiment; it is a definite system of testing, worked out on a carefully prepared plan. Instead of happy-go-lucky, lazy observation, there is active systematic participation in each case.

The result of such an experiment on any given individual determines on general principles *what* colours he can see. It is even possible to re-construct the world of colours as it appears to him, but in general outlines only.

How can an advance be made? In statements concerning the results of these tests, no references to quantity can be introduced. The red-blind person may see red as dark green, yet we cannot give any account of just how dark a green corresponds to each particular hue and intensity of red. For an advanced knowledge of this kind, where the hue and shade of each colour are exactly determined, we must introduce measurements into the experiments.

[1] See the chapter on Colour and Appendix VI.

Beginning with Maxwell's colour-top, and advancing through the spectroscopic work of Helmholtz and König, whereby the psychology (not physiology) of colour-vision in its various forms has been reduced to an exact science, our knowledge and views of the subject have become profoundly modified by means of measurements, as will be explained in Chapter XXIV.

Having seen how, by a steady progression, cursory observations may lead to tests, then to qualitative experiments, and finally to measurements, let us turn to the far more difficult case where an investigator introduces scientific methods of experiment directly into an entirely unexplored region.

"Before the introduction of experiment, our knowledge concerning memory consisted of vague generalities, such as 'quick to learn, quick to forget,' 'old age forgets the most recent events,' 'strong attention gives a good memory,' &c., &c. These generalities were illustrated and supported by our every-day experience, and by numberless anecdotes of strange freaks of memory. The pathology of memory was mapped out in the broad lines of general and partial amnesia, hypermnesia, &c. Such outline knowledge is very useful in practical life, and not a word is to be said against it or against those who spent their time in compiling it. But it is not scientific knowledge. If scientific or practical curiosity inquires, how does forgetting depend upon the elapsed time? how does the quality of memory depend upon the number of repetitions? what is the influence of different degrees of interest? &c., it finds no answer."

The story that we find in Ebbinghaus's attempt to investigate memory[1] is so typical of the experience that every pioneer undergoes in forcing a way into an untrodden wilderness, that I shall tell it in brief.

[1] Ebbinghaus, "Ueber das Gedächtniss," Leipzig, 1885.

The inability to answer such questions concerning details, says Ebbinghaus, does not arise because by chance these details have escaped investigation, and can be determined whenever we have a mind to do so. On the one hand, although we feel that such ideas as degrees of forgetting, of certainty and of interest, are quite correct ones, we find no means in our experience of defining such degrees except in the wildest extremes and vaguest indefiniteness. We consequently have nothing to start an investigation with. On the other hand, we find that our ideas of memory, formed for certain extreme cases, do not fit the facts of memory in ordinary life, and we also find that we have not even grasped many fundamental ideas necessary to a study of the details of the facts, and to the theoretical grasp of the results. Such metaphorical concepts as the usual ones of stored-up impressions, of more or less travelled paths, of images engraved on the mind, &c., have only one thing certain about them, namely, that they are entirely inappropriate.

The possibility of gaining any accurate and trustworthy knowledge concerning memory depends upon the possibility of applying what is often called the "natural science" method. This is the method on which all science must rest, and is called the "natural science" method merely because mental science has made so little use of it. This method is essentially as follows: from the complexity of conditions surrounding a phenomenon we first exclude those that are evidently unessential; then we seek to maintain all the others unchanged except one; we vary this one by definite steps, and determine what changes occur in the phenomenon.

How is this to be done for such an indefinite thing as memory?

In the first place, we must find some property of the phenomenon to be remembered, which admits a numerical definition. We can, for example, take a composite phenomenon, say a set of syllables, and give the proportion of remembered syllables. This would make the experiments statistical; some examples have been given in the previous chapter. Or we can determine the number of repetitions necessary for first learning the phenomenon, *e.g.*, a set of syllables, and the number necessary for rehearsing it as a remembered event. This makes the experiments into measurements; it is the method of Ebbinghaus.

In the second place, we must get control of the surrounding conditions; then we must find a way of making true experiments. This is the point in which we are at present specially interested.

The complicated manifestations of the human mind as exhibited in historical and political life would in general lead us to deny any constancy of conditions of mind. Caprice and irregularity are the chief characteristics exhibited. We ourselves feel how changeable are such important factors as mental freshness, interest, degree of attention, sudden changes in the course of thought, &c. Nevertheless, we can do something in the way of reducing these conditions to regularity. For example, memorising pieces of prose or poetry with their varied thoughts, metaphors, rhythms, and rhymes, would have brought into play endless associations, degrees of interest, memories, &c. All this Ebbinghaus avoided by using sets of meaningless syllables, which in thousands of combinations produce a meaning on only a few occasions. Again, to keep the mental freshness as constant as possible all great changes in the manner of life were avoided, and the experiments were performed at the same hour of the day. Changes in the course of

thought were avoided by repeating the syllables in constant time to the ticking of a watch, by compelling the maximum of attention in the effort to learn the set as quickly as possible, and by strict avoidance of all external distractions. (An account of the results will be found in the chapter on Time Influence.)

To the astronomer, the physicist, or the chemist, this explanation of the nature of an experiment will undoubtedly seem a needless statement of what everybody knows already. The feeling must be like that experienced by a mathematician on reading the first chapter of Clifford's "Common Sense of the Exact Sciences." Yet there are many persons to whom these methods of thought are unknown. Simple as the principles of experimenting may be, there are many persons claiming to make experiments on mental life, who have never grasped the idea of a true experiment, namely, that, in order to be reliable and scientific an experiment must be systematically carried out on a pre-conceived plan, with an elaborately minute study of all conceivable sources of error.[1]

To bring out the contrast between a trained scientist and an untrained one, I will take a couple of examples from the domain of psychical research. In reading the accounts of "thought transference" we find that the conclusions are based on "experiments" of the following sort: "At first the percipient (person experimented upon) sat facing the agent, but after about 1000 trials had been made her back was turned to the table." One thousand experiments were made before a flagrant source of error was corrected. "About 1000 experiments" reminds us of the accuracy of cook-book science, "about" a pound of butter or a handful of

[1] Wundt, *Selbstbeobachtung und innere Wahrnehmung*, "Philos Studien," 1888, iv. 1292.

sugar. Other "experiments" are reported in this style: "Twenty-five experiments were made, of which, unfortunately, I have kept no record, except of the following three." "Then follow thirteen trials with fantastic figures, details of which Dr. Ochorowicz does not record." "When asked what object, named Pincushion" (with no record of how the question was asked). "We have now a record of 713 experiments, and I recently set myself the task of classifying them into four classes of unsuccessful, partially successful, misdescriptions and failures. I endeavoured to work it out in what I thought a reasonable way, but I experienced much difficulty in assigning to its proper column each experiment we made." Of course, the column of "successful" received the largest number. In another case "it is believed that the precautions taken were in all cases adequate to conceal the object, &c," and so on. Imagine a shopkeeper who buys *about* 1000 chairs before noticing that they have no seats, or who sends out 25 consignments of cutlery, but keeps no record, and sends in the bill for only the last three. Imagine an electrician making "thirteen trials with fantastic machines," and daring to publish the results.

Suppose, however, that we consider the question of thought transference to be of sufficient earnestness to justify a practical trial by scientific methods. It has been so considered by Hansen and Lehmann,[1] and the methods employed are so strikingly illustrative of the difference between scientific experiments and unscientific ones, that a brief account of them will bring out the contrast.

If a thought in one person's mind is transmitted into that of another person, the transference of energy must

[1] Hansen and Lehmann, *Ueber unwilkürliches Flüstern*, "Philos. Studien," 1895, xi. 471.

have occurred in some way. Let us suppose it to resemble the transmission of sound in the air, or of light and electricity in the ether in being a vibratory movement of some known or unknown medium. Now all hitherto investigated vibratory movements are reflected from metal mirrors, and this new one will probably be no exception to the rule. By means of concave mirrors, therefore, we may expect to concentrate the thoughts strongly to a focus, so that the experiments, which now succeed only occasionally, may succeed in large numbers whereby we can hope to study their laws. Lehmann, a psychologist, and Hansen, a physician, prepared two concave metal mirrors with 54 cm. radius of curvature; the size of the mirror was such that the focus lay in the plane of the opening, the diameter of this opening being 90 cm. The mirrors were placed opposite each other, with their axes falling in the same straight line and their foci distant by 2 m. Each of the two persons sat with his head in a focus, the face being turned to the mirror. The experiments were made with numbers of two figures each contained as counters in little bags. The experiments were carried out in the laboratory, mainly in the morning when there was almost perfect quiet in the building. It was impossible for either person to see directly or indirectly the number drawn from the bag by the other. No other persons were allowed in the room, in order to exclude the possibility of involuntary help.

The percipient took care to suppress all capricious thoughts so that he could await the coming of the pictures with a mind as empty of thought as possible. After five to ten minutes, pictures of numbers actually began to arise in consciousness; when they had attained a certain definiteness they were written down, and, after

that, were compared with the numbers of the counters drawn. In this way fifteen experiments were made with unexpected results. Six of them succeeded, with results as shown in Fig 1; the lower line gives the results copied exactly after the original records, and the upper line gives the corresponding numbers of the counters.

Here we have a most striking experimental confirmation of the fact of thought-transference. We might, like the psychical researchers, proceed to calculations of probability, *e.g.*, if a counter be drawn by chance from the total of 90 counters, the probability of drawing any particular one is 1-90, and likewise the probability of recording at random any particular one of 90 possible figures is 1-90. Now, the probability that the two

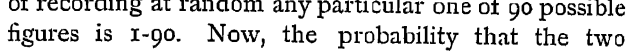


Fig. 1. EXAMPLES OF THOUGHT-TRANSFERENCE.

agree by chance is equal to the product of the separate probabilities, or 1-8100. Only once out of 8100 times ought an experiment to succeed. Here we have six successes out of fifteen experiments; and in this case the experiments were carried out by trained men with absolutely no possibility of mistake or collusion.

Let us proceed with the account. The pictures are, as you see (say the authors), very indefinite. With the intention to do so, you can make them mean any figures you please. It often happened that the figures were read differently by the two experimenters. The first number was read as 16 by the percipient; the other turned the paper around and the number appeared plainly as 77, which was the number he had drawn and thought about. As this was the first successful

experiment of the series, the authors sought a theory to explain the inversion of the thought-pictures by concave mirrors, but were obliged to give up the attempt as the case did not occur often. Moreover, the subsequent experiments soon taught them that all that was necessary to success was the intention to find a resemblance between the pictures and the figures. Figures are made by a few combinations of straight and curved lines, and, when the written figures in such thought-transference experiments are made indistinctly, it is not difficult to find a resemblance between them and the originals. In short, the coincidence of the results was a pure illusion of prejudice, as the critical reader can certainly convince himself by a study of the figure. Lehmann concludes: "For all illusion it is the law that the similarity of the unknown, or of the indefinitely perceived to something known, is overestimated. And such an overestimation is unquestionably the case when an equivocal picture is construed in the sense of agreement with the original number. Only when we wish to deceive ourselves at any price can we look upon such results as a proof of thought-transference."

On one occasion the experimenters tried the transference of drawings. After about ten minutes a picture arose. "I drew Fig. 2A," says the percipient, "and handed it to the sender, who found an undeniable resemblance between it and his original, Fig. 2B. A resemblance is there, to be sure; but unfortunately I did not intend the drawing for a candlestick but for a cat."

This case, continue the authors, seems to be quite noteworthy. Hysterical or hypnotised persons are the most frequent percipients in such experiments. If a hysterical person had made such a drawing, the

conductor of the experiment would have regarded it as successful, and in consideration of the character of hysterical people it would scarcely be expected that the percipient would have called attention to the illusion. We can safely assume that the examples of so-called thought-transference have in many cases arisen in just this way. If we look through the hundreds of drawings in Richet's work, and in the Proceedings of the Society for Psychical Research, we readily see that the resemblance of the two drawings is in only extremely few cases more than in the one given. But if the

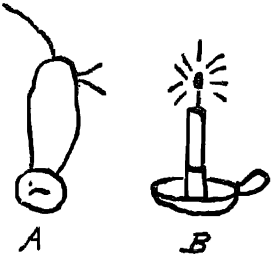


Fig. 2. A CASE OF THOUGHT-TRANSFERENCE.

badly drawn picture of a cat is considered similar to a candlestick, it is hard to see why the case is not the same with the hundreds of "successful" experiments.

Take, for example, one of Richet's cases. The somnambulist begins: "A basin with a fountain in the middle," and makes the drawing, Fig. 3A; "in the middle I see a kind of holder to stick flowers in, that is straight like a stick." Then she makes a success in the drawings, Fig. 3 B and C. The original was a crab.

We can, perhaps, find a distant resemblance between Fig. 3C and a crab with outspread claws; but the percipient had not thought of a crab at all. The picture of a fountain was in her mind, and this she had

indicated by a few rude lines. No one would think of a crab on looking at this drawing; the drawing might just as well indicate a table with flowers, or a squirting whale coming up from the depths of the ocean. If the original had been such a drawing, a resemblance would unquestionably have been assumed. And this is what Richet regards as a "successful" experiment, a proof of thought-transference and clairvoyance.

But I shall go no further in this account of Lehmann and Hansen's experiments and conclusions.[1] I have, I hope, said enough to make clear what an experiment is and what it is not. Such an explanation seems

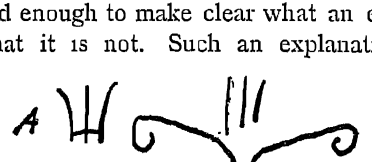


Fig. 3. DRAWINGS BY THOUGHT-TRANSFERENCE.

necessary at a time when so many really educated persons have put their faith in the results and deductions by the methods of psychical research. It is *a priori* impossible for an untrained man to make scientific experiments, and it is to be deeply regretted that persons of distinction in other lines should undertake problems that require all the skill of a long-trained worker in the psychological laboratory.[2]

[1] These preliminary experiments led to a valuable investigation of unconscious nasal whispering, which will be briefly referred to in the chapter on Movement.

[2] Any one who wishes another example of the blindness of the untrained "experimenter" will find the story in Hart, *The Revival of Witchcraft*, "Nineteenth Century," 1893. An exposure of the wrong methods of various societies for psychical research, and "experimental (?) psychology," can be found in Wundt, *Hypnotis-*

The objectionable feature of psychical research does not lie in its subject of investigation. Mesmerism led to our present knowledge of hypnotism ; thought-reading has stimulated investigations on the unconscious movements of the arm dependent on attention ; and thought-transference has brought about the discovery of the possibilities of nasal whispering. The objections to psychical research lie in its unscientific methods of experimentation and in the air of occultism in which the whole is enveloped. If the investigators were trained in the psychological laboratory, we might expect interesting discoveries in regard to mind, while at the same time the repellant mysticism would disappear along with odic force, animal magnetism, thought-transference, and other ghosts.

The sources of error in experimenting are so manifold and insidious that their avoidance and elimination has become an art which can be learned only by a specialist. A careful systematic study of these sources of error must be made by every psychologist.

For the purpose of training the workers in the laboratory I have prepared a classification of the most important sources of error in experimenting and measuring. The chief sources are found in the degree of attention, in pre-disposition, in fatigue, in practice, in the apparatus, in the unit of scale, in estimation of the sub-unit, in computation, in definition. The student must receive a careful training in their detection, elimination, and compensation. Only a thoroughly trained investigator can be decently certain that he has not committed every one of them in such a high degree as to make his results worthless.

It is by the continuously improved study of errors

mus und Suggestion, "Phil. Stud.," 1892, viii. 1 ; also separate, Leipzig, 1892.

that science advances. What is thrown aside as an error, or is regarded as negligible at one stage of development, furnishes the material for the next. The rule of the past is the rule for the future: do not throw away your residues, but look for your discoveries there. I will illustrate this important law by showing how the study of certain errors or residues has lead steadily to new knowledge.

Let us take such a simple matter as finding the relation (without regard to measurements) between our sensations and bodies in different conditions of physical heat.

We all know that some objects feel hot, some cold, and some indifferent. Physicists tell us that the molecules of these bodies are in different conditions of motion. If the molecules could be brought to rest the ideal thermometer would stand at the lowest point possible; as the motion increases, the thermometer indicates a higher temperature.

In the first place, we quickly notice that all bodies can be psychologically classified as warm, indifferent, or cold. Rough observation tells us that bodies which are physically of a high temperature, for example, a red coal, are also psychologically hot; and that bodies physically of a lower temperature, for example, ice, are psychologically cold. Also, roughly speaking, when a body passes from a low temperature to a high one, the sensation changes from cold through indifference to hot. These observations would seem to indicate that our sensations of warmth and coldness run an even pace with changes above and below a point of indifference. It is apparently a general law.

But have we overlooked anything? We begin to experiment on our sensations by applying hot and cold bodies; this calls to our attention the fact that different parts of the skin may not agree in their

results, and we proceed to investigate the residual phenomenon previously neglected.

Simple trials with bodies of various temperatures soon make it evident that for extremes of high and low temperature there is substantial agreement all over the skin, but that for moderate temperatures the same object will sometimes appear cool on one place and indifferent on another, warm on one place and indifferent on some other. It is evident that coolness and warmth do not depend alone on the temperature of the object, but also on the circumstances under which it is applied.

Taking up this source of error as a matter for investigation, we find that for any given place on the skin there is one temperature that can be considered as an indifference point (the "psychological zero"), and that bodies lower in temperature than this point appear cool, and those higher appear warm.

But various experimenters use objects of various areas; has the area of the applied object any influence? This starts a new investigation, with the result that we find the sensation to depend on the area. For example, the end of the finger can be comfortably held in water that is intolerably hot to the whole hand. From this point we might proceed by measurements to determine the law of relation between area and intensity of sensation.

If an object of very small area, *e.g.*, a pointed nail is used, a new phenomenon appears. The sensation arises only at certain points on the skin, and not everywhere equally. It is also soon noticed that there is a separate distribution of the hot spots and cold spots (as we may call them).[1] We can say that from certain spots we get sensations only of warmth, and from others we get

[1] Goldscheider, *Ueber Wärme-, Kälte-, und Druckpunkte*, "Du Bois-Reymond's Archiv. f. Physiol.," 1885, 340.

sensations only of coolness. We would naturally say that hot spots respond only to warm objects, and cold spots only to cold ones. But a source of error remains. If we use objects of extreme temperature, we find that a cold spot will often answer with a sensation of coolness even when the body is very hot. (I have never observed the reverse.) It is also observed that pointed bodies with a temperature not far from indifference are often answered by sensations from both hot and cold spots, but that although these are clearly *sensations of temperature*, they are not distinguished into those of warmth and coolness. For example, when drawing lightly over the hand a pointed lead-pencil taken from the pocket (*i.e.*, with a carbon point of about the body temperature), I can locate certain prominent temperature spots, and can feel that the pencil has some temperature to it, but cannot say whether it feels slightly warm or slightly cool.

We have successively limited the problem to more definite circumstances. Probably we have gone as far as would be profitable without introducing very accurate measurements, yet a final solution is just begun. We need to take into account the temperature of the indifference point, the relation of this temperature to that of the body applied, the length of time of application, the amount of heat transmitted in that time, the quality of attention on the part of the person experimented upon, &c.

Up to this point in the chapter I have confined my illustrations mainly to what may be called qualitative experiments; science never stops at such experiments. Scientific experiments in the highest sense of the word are made up of measurements. Qualitative experiments are necessary for preliminary investigations, but they are inexcusable where quantitative

ones can be made. That is to say, although they are necessary as forerunners of experiments with measurement, and although at certain stages of investigation they are of incalculable value, yet the scientist may never rest satisfied with them, but should regard them only as stepping-stones for future progress.

"When you know what you are speaking about, and can express it in numbers you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science." [1]

Suppose that our object is to determine the effect of hearing tones on the power of muscular exertion. We may first make some preliminary qualitative experiments. For example, some object is found that can be lifted only with difficulty by the subject of the experiment. The conditions of grasp, practice, and fatigue can readily be made fairly constant by the usual methods familiar to psychologists.

We may get three varieties of results. (*a*) If on repeating the experiment many times we always get a difference for the sound the case is proven, and we proceed to obtain more definite knowledge by means of measurements. (*b*) If on repeating the experiments we get irregular results we may proceed to take statistics, and from a large prevalence of one kind of result over the other we might be able to draw a positive conclusion. A better way, wherever possible, is to proceed to measurements instead of statistics. (*c*) If we always get about the same result with or without a tone we can draw the conclusion that no

[1] Thomson, "Popular Lectures and Addresses," i. 73, London, 1889.

difference great enough to be detected by our method exists. With qualitative experiments we are unable to assign any size to this uncertainty, by proceeding to measurements we can exactly define it, and can say that no difference greater than a definite amount is produced.



Fig. 4. EXPERIMENT WITH THE DYNAMOMETER.

It is evident that for any really definite knowledge we must proceed to measurements.

We first take one particular case of the general problem ; say the greatest possible exertion for a thumb and finger grip. The dynamometer, Fig. 4, is held between

the ends of the thumb and index finger. The brass back rests lightly, without pressure, on the palm of the hand. The scale of pressure in grammes or kilogrammes is marked on the scale-plate. As the thumb and finger are made to exert a pressure, the point moves over the scale ; the reading shows the pressure exerted. Silence being observed, the knobs are pressed as strongly as possible. The index stands at a certain mark, say 6 kilos. A loud tone is now produced on a violin, and, while it is sounding, the knobs are again pressed. If it goes down on the scale further, or not so far, the result would seem to indicate some influence of the tone.

The scale has already been graduated by weights. Its own uncertainty is probably about 10 grammes. If our measurements show no difference over 10 grammes that amount is the extent of uncertainty for the statement. If our measurements lie irregularly over the scale, we place the results of those made in silence in a separate column from those made with a tone. The mean of each set is found, and mean variations are calculated (p. 47). If there is no difference between the averages, we say that within a limit of uncertainty indicated by the mean variations there is no effect. If there is a difference, we can say that, with this range of uncertainty, there is an effect of an amount equal to the difference between the two averages.

With these measurements one single fact has been determined ; one single quantity has been measured. The whole forms a single quantitative experiment. For some purposes we might stop here, but for a scientific result we must proceed to execute a *system* of quantitative experiments, or an investigation.

In our problem there are many factors linked together ; we must determine the numerical relations between each possible pair.

In the first place, the loudness of the tone may make a difference. Everything else being kept just as before, the experiments are performed with tones of different loudness. For a tone with the loudness a, we get a grip of m kilos.; with the loudness $2a$, we get m' kilos.; with $3a$, m'' kilos., &c. In this way we establish a definite relation between the intensity and the grip under the particular conditions. The fact that such a relation is or can be found we express by saying that the grip is a function of the intensity, or briefly $m = f(a)$. The arbitrarily varied quantity is termed the "independent variable"; the other is briefly termed the "function."

The pitch may have some effect. Therefore we must try tones of different pitch, but of the same intensity. Suppose we denote the pitch by the vibration-number or frequency for each tone. Then we get a definite relation between grip and pitch. Grip is a function of pitch, or, if b denote the successive units of pitch, $m = f_1(b)$.

It might also happen that the grip would change with the length of time the tone sounds. Grip may be a function of the duration of the tone, or, if c denote the duration, $m = f_2(c)$.

Summing these all up we have the force of grip dependent on intensity, pitch, and duration of the tone, or $m = F(a, b, c)$.

When we were investigating $m = f(a)$, we kept b and c carefully unchanged or constant. When we tried $m = f_1(b)$, we kept a and c constant. Not only that, we tried to keep everything else as nearly constant as possible. Various other phenomena might influence the grip, *e.g.*, time of day, humidity, age, attention, emotion, particular finger, particular muscles, &c. All influences that might possibly affect the result, except the particular one investigated, are supposed to have been kept constant.

Of course this cannot be perfectly done; disturbances, therefore, cause a slight uncertainty in the result, but we have the mean variation as a measure of all the uncertainty. If the uncertainty is satisfactorily small, we can assume that the conditions were constant enough for the purpose.

To satisfactorily complete the investigation we should try the effect of every suspected influence. The concept, function, is extended to include cases where no effect is produced by the variations of an assumed influence. We can thus in general say $m = \mathrm{F}(a, b, c \ldots r, s, t \ldots)$ where the letters and the dots indicate all possible influences.

To measure the grip under all imaginable circumstances is, of course, impossible. In practice we select those that promise some result, and treat all the rest as merely contributing to make up the uncertainty. As soon, however, as a demand for less uncertainty is made, some of these neglected quantities must be investigated. Science advances by investigating previously neglected phenomena; it is obliged to advance by the demands of greater accuracy.

The fundamental principles of a quantitative investigation can be stated in this way:—

1. The quantity to be investigated is considered as a function of all possible quantities: $m = \mathrm{F}(a, b, c \ldots)$.

2. All these quantities, except one, are maintained unchanged as nearly as possible or profitable throughout one section of the investigation, while that one quantity is treated as the independent variable.

3. This process is performed in all possible or profitable ways:—

$$\begin{array}{rcl} b, c, \ldots = \text{const.}, & m = \mathrm{f}\ (a) \\ a, c, \ldots = \quad ,, & m = \mathrm{f}_1\ (b) \\ \vdots \qquad\quad \vdots & \vdots \quad \vdots \\ \ldots r, t, \ldots = \quad ,, & m = \mathrm{f}^r\ (s) \\ \vdots \quad \vdots \qquad\quad \vdots & \vdots \quad \vdots \end{array}$$

4. The unavoidable changes in the quantities assumed as unchanged, and the inaccuracies in the quantities measured, are treated as sources of error, the result of all these sources being an irregularity in the values of *m* obtained for any given value of the independent variable. This irregularity of the results is a necessary and desirable feature of a set of measurements (p. 45).

In conclusion, we have seen in this chapter that experimenting is a great advance over mere observation, that scientific experiments can be made only by trained scientists, that the highest type of experiment is one in which the data are determined by measurement, and that the final aim of scientific work is the establishment of general laws by systematic variations of the experiments.

PART II.

TIME.

CHAPTER V.

STANDARDS OF TIME.

WHEN you listen to the noises coming in from the street you notice that they do not remain the same. Footsteps grow louder or fainter, the wind rises or falls, voices arise and cease. There is no monotony, there is a never-ceasing change.

Let us attempt to remove all change. We enter an isolated room where all sounds can be kept out; we hold a telephone to the ear and listen to the monotonous tone from an electric tuning-fork in a distant room. There is no change in the sound, and we find it difficult to estimate how long we have stayed in the room.

Not only sounds, but all our experiences change. Tastes and smells come and go. Backaches, headaches, pressures, pains, hotness and coldness, sights of all sorts, likes, dislikes, and impulses of various kinds arise and pass. This particular kind of change, which is common to all experiences, is what we call "time."

We have "time" for every variety of simple and

complicated experience. Time with rapid changes of sights is quite a different thing from time in darkness. Time when listening to music is different from time when eating dinner. Can we not find some standard from which we can reckon all our particular times as variations?

Among our experiences we find some that recur on many occasions. Let us take the sum-total of change between any two such occurrences as a standard of time. Intervals between showers would not make good standards, as we find that the sum-totals of change would be very different for different intervals. Day and night would serve very well, for one day of life contains about as much change as another. The uncivilised man or rude peasant might well rest content to count his life from noon-day to noon-day. For larger periods he will naturally accept such occurrences as the moons, the seasons, the flood of the Nile, &c.[1]

To subdivide the standard day man uses the clepsydra, the burning of a candle, the continuous chanting of psalms in the monastery, the swing of a pendulum, or the movement of a balance-wheel.

How do we regulate the equality of the sub-divisions? Water-clocks and pendulum-clocks can be arranged to drop equal quantities of water or perform equal numbers of revolutions to indicate hours. Of course, some clocks will run closely together, and others will scatter. The more carefully we construct them, the better they agree. We eliminate the disagreeing ones, and accept the records of the agreeing clocks as correct. By comparing pendulums of different length with clocks that subdivide the days into hours and minutes we can pick out the pendulum that beats sixty times a minute. We

[1] Wolf, "Geschichte der Astronomie," München, 1877, i. cap.

now have as standard the solar day, and as unit the second.

Long ago it had been noticed that the revolutions of the stars around the earth differed from that of the sun. Accurate clocks would soon make it evident that the sidereal day is more regular than the solar day. The standard then becomes the sidereal day, and the unit termed the second is the $\frac{1}{86164}$ part of the sidereal day. As the sidereal day differs from the solar day, and as for practical reasons the latter must be used, the clocks are adjusted to the average solar day, of which the second is the $\frac{1}{86400}$ part.

Theoretical considerations lead us to believe that, if we could construct apparatus fine enough, and could make observations extending over many million years, the sidereal day would be found to have become longer than what our apparatus would indicate as the proper amount. At the present moment, however, the rotating earth, by which the sidereal day is determined, is the most accurate instrument we have. The adoption of the sidereal day as a standard thus introduces the maximum of agreement for intervals of change.[1]

This standard time is what we mean by *absolute* time. When we hereafter speak of an interval of time we shall mean an interval of this time. If we are looking at an accurate clock, an interval of one minute will mean the change-interval of one revolution of the seconds-hand. If we then look at the street for an interval which we judge equal to the interval passed in looking at the clock, the two intervals are equal as far as we are personally concerned, but they may not be equal when judged by standard time.[2]

[1] Jevons, "Principles of Science," chap. xiv., London, 1887.

[2] This is the problem investigated in the chapter on Time Estimate.

For hearing we take as standard intervals the ticking or the striking of a clock. For touch we would use successions of blows on the skin, the blows being derived from the clock.

In all this consideration of time we do not go outside of our direct experience. Time is a property of all our mental processes; standard time is an abstraction whereby we obtain a maximum agreement among the times for each particular kind of experience.

If time, as judged from a succession of sounds, of sights, of memories, of impulses, &c., is a mental matter, then standard time, the system of greatest agreement for them, is also a mental matter. When we compare our estimate of time, as judged by ear, with time-records as indicated by clock, we are simply comparing a particular case of mental time with a more general case. We are not stepping over the boundary from a mental time to a physical time, but are comparing two purely mental quantities.

Experiments on time require a carefully arranged system of apparatus beginning with the rotating and revolving earth, and proceeding step by step downward to vibrating bodies adjusted to the ten-thousandth part of a second.

Starting with our biggest piece of apparatus, we must have some means of measuring one complete rotation of the earth. A telescope is pointed toward some star. The star passes over the central hair-line of the telescope. If the telescope be left unchanged in position, every following passage of the star across this line will mark one complete rotation of the earth. This gives the sidereal day from which the mean solar day can be calculated.

To subdivide this interval we use a carefully constructed clock, so arranged that its hour-hand makes

two complete revolutions for one rotation of the earth. The clock is adjusted and tested by comparison with the passage of the star till the agreement is close enough for our greatest need. We thus transfer the day-interval from the earth to a more convenient apparatus—the clock.

Long experience has taught the clockmakers that in carefully made clocks the motion of the hour-hand is sufficiently regular to render it possible to subdivide the whole into parts; thus the hour intervals are obtained. Similar care and experience render it possible to add another hand and obtain the minutes.

We must now subdivide the minutes into seconds. A minute on one of our clocks is really one revolution of a toothed wheel, whereby each tooth counts off one swing of a pendulum. The clock simply counts up the number of swings per minute, per hour, per day, &c. Therefore, if we can have the count come out just right at the end of a long time, it is evident that the pendulum must have swung the correct number of times. If the clock is ahead in its count we lengthen the pendulum; if it is behind, we shorten it. Finally we get it just right. For a very carefully constructed clock we can suppose that all swings of the pendulum are equal. Consequently when we get the total just right, each second is definitely marked off by a single pendulum swing.

It is necessary to render the seconds available at any place and in any way. This is done by the clock contact. We attach a metal contact so that an electric circuit will be closed for an instant whenever the pendulum passes its lowest point. Such a pendulum contact, metal against metal at the lowest point of swing, is shown in Fig. 5. We send a current through this contact and through a spark-coil in such a way that an electric spark is produced every time the pendulum

passes the lowest point. We see sparks with intervals between them. By direct comparison we can see that the sparks occur at the time the pendulum passes its lowest point. Now we make two assumptions—that the time between any two passages of the pendulum is always the same, and that the sparks occur at just the

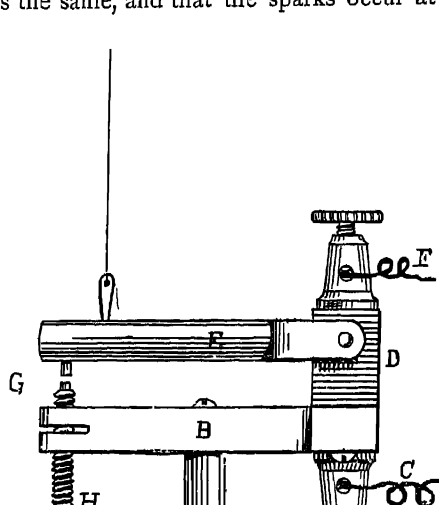


FIG. 5. PENDULUM CONTACT (ENLARGED).

corresponding moments of the swing. Neither is exactly true, but by experimental means we can prove that for a properly made clock and a properly adjusted contact the error is so small as to be entirely insignificant. By such electrical arrangements we can get the second whenever and however we may want it.

We have just obtained a series of sparks at inter-

vals of one second; for our finer measurements we must split this second up into tenths, hundredths, and thousandths. Let us begin with tenths. We need two things, a recording apparatus and a vibrating body.

The graphic method of obtaining records of time has been derived from astronomy but has been modified for psychological needs. We have adopted and developed the method originally used by Thomas Young, that of a rotating cylinder with a covering of smoke. The usual cylindrical recording drums made for astronomy, physics, and physiology do not exactly suit psychological work; it is about the same ill-fit that comes with ready-made clothing. I have therefore devised a drum specially adapted to psychological needs.[1] The cast-iron base, Fig. 6, is supported on three fixed and one adjustable leg. The drum runs on hardened steel centres held by two uprights bolted to the base. Around this cylindrical drum a strip of glazed paper is stretched and fastened with paste at the ends. A gas jet is held under the drum in such a way that it deposits soot on the paper. By slowly turning the drum the paper can be completely coated. A vertical support opposite the drum is used to hold small recording instruments in such a way that the fine pointers just touch the paper. When the drum is turned each draws a fine white line in the soot. A handle at the end turns a screw and enables the experimenter to draw the support sideways. The drum may be rotated at any desired speed by a belt from a small electric motor, series wound, whose speed is governed by the strength of the current. Direct rotation by the hand is, however, the most convenient method.

We first proceed to divide the second into hundredths.

[1] Scripture, *Some New Psychological Apparatus*, "Stud. Yale Psych. Lab.," 1893, i. 97.

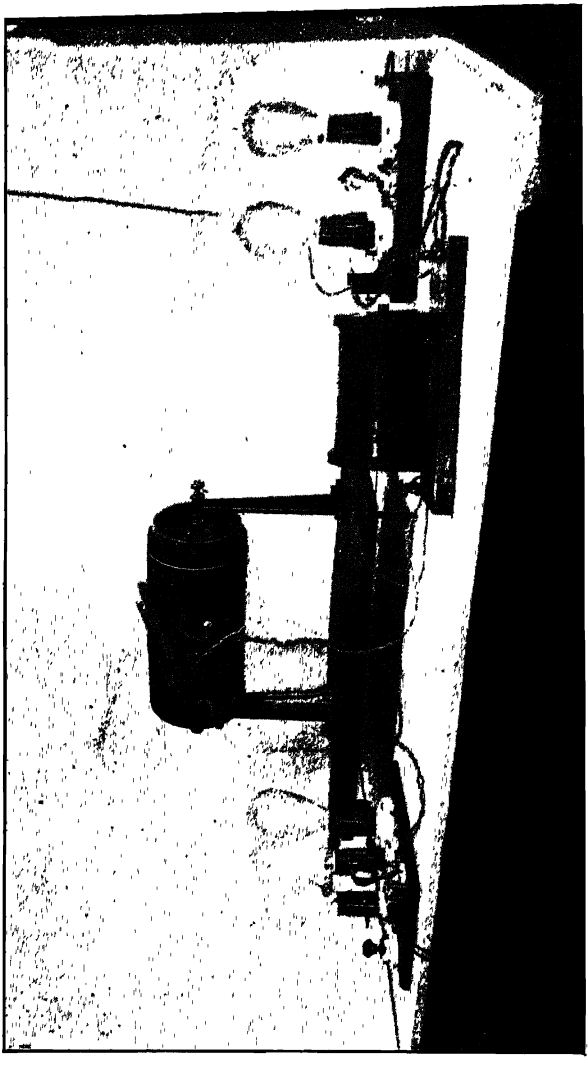


Fig. 6. ARRANGEMENT FOR DIVIDING A SECOND INTO HUNDREDTHS

A large tuning-fork, about a foot long, is arranged to vibrate automatically by means of an electric current and a magnet; the principle involved is the same as that of an electric bell, or any self-interrupting magnetic vibrator. This fork is placed on the vertical support in such a way as to cause a fine metal point attached to one end to rest lightly against the smoked paper of the drum. As it vibrates it draws a wavy line on the revolving drum.

To get a record from the pendulum, an electric current is arranged to pass from a battery through the pendulum contact and the primary circuit of a spark coil. One pole of the secondary-circuit of the spark-coil is connected to the base of the drum, the other to the fork.[1]

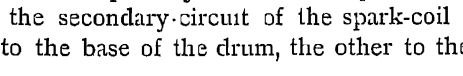


Fig 7. A SPARK RECORD.

The result is that whenever the pendulum swings past its lowest point, it closes and breaks the primary circuit momentarily, and a spark snaps from the fine point of the fork through the paper to the drum, making a white dot, as indicated in Fig. 7.

If the fork is vibrating exactly 100 times a second, any two successive sparks will be separated by exactly 100 waves. If the distance is filled by more than 100 waves the fork goes too fast; if by less, it goes too slowly. It is adjusted by regulating a light weight on one of the prongs till it vibrates exactly 100 times to the second.

To still further divide the second, instead of using

[1] The fork is run by the lamp-battery at the left in Fig. 6; the battery for the coil is at the right. The wires from the pendulum come from above. For the construction of the lamp-batteries see Appendix IV.

smaller forks (whereby the counting of records would become laborious) we take advantage of the evenness of the waves from the 100 fork. By rotating the drum with sufficient rapidity these waves are made long enough so that each can be divided by the eye directly into tenths. Thus we obtain the thousandths of a second.

In this whole process we have not stepped outside the sensations of sight; the swinging of the pendulum, the series of sparks, the vibrations of the forks, and the records on the drums were all mental phenomena. We *see* a series of dots where the interval of space between any two represents a time of one second. We also *see* each space divided into 100 or 1000 equal parts, and are justified in assuming that each of these parts represents 0.01 or 0.001 of a second. These are the units we shall use for measuring mental phenomena in regard to time. Of course, this does not mean that the apparatus and the time are not physical also. The only justifiable standpoint for an experimental science to assume is that of the physical sciences, namely, the reality of our sensations. The apparatus and the intervals of time are assumed to be real things and real events.

CHAPTER VI.

TIME OF SENSATION.

TIME is required for seeing, hearing, or otherwise experiencing any sensation. A treatment of the time of sensation will include a study of its appearance, continuance, and disappearance.

We will first consider the time required for appearance, or, as it is called, its latent time.

For experiments on sight the apparatus shown in Fig. 8 may be used.[1] A heavy block of iron ss is held up by an electric magnet M. In this block is an opening A; on the front is a grey patch F. The objects to be seen are slid just behind F on the carrier K. The front of the block is covered by paper, black or white according to the experiment.

The observer looks at the point F. The current going through the magnet is broken; the iron block falls downward rapidly and accurately along the rails at the sides. As the opening A passes the object behind F, it is seen for an instant. The length of this instant is regulated by the width of the opening A. By adjusting A any desired small time is obtained.

In one set of experiments a piece of coloured paper is placed on the carrier K. The falling block is covered

[1] Cattell, *Ueber die Trägheit der Netzhaut und des Sehcentrums*, 'Phil. Stud.," 1886, iii. 94.

with white paper. The opening A is made very small. The block is dropped. The observer can detect no colour. Then the opening is made wider. Again no colour is detected. Finally, the opening is made just wide enough for the colour to be always detected. Since the rapidity of the falling block is measured, the exact time during which the colour was exposed can be accurately calculated from the width of the opening.

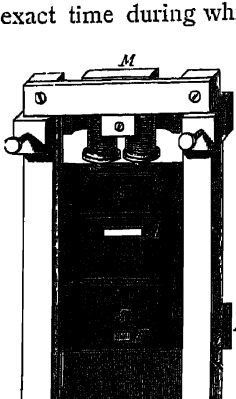


Fig. 8. APPARATUS FOR MEASURING THE LATENT TIME FOR SIGHT.

A typical series of results gave the following as necessary for sensation of the particular coloured papers: red, 8σ; orange, 5σ; yellow, 6σ; green, 8σ; blue, 8σ; violet, 13σ ($\sigma = 0.001$ sec.). These results hold good only for the particular paper by daylight. For other pigment colours or for spectrum colours they would vary more or less.

These intervals of time represent two processes. The coloured paper is first seen; then the impression is replaced by white. But since there is also a latent time for the white, the colour must have been present actually longer than it was exposed. The experiments really measured the difference between the latent time of the colour and that of white. We shall not be far wrong if we assume that the latent time for a colour is twice that for white. Consequently we must double the figures in order to find the latent time for a colour. This is speaking rather roughly, but as the colours used were from coloured papers instead of

spectrum colours, and as there are no measurements of their relative photometric intensities, we are not justified in particularising further than to say that the latent time for a colour lies somewhere between 10σ and 20σ. Whether or not it depends on the particular hue of colour, when all the colours are made of equal energy, the results do not enable us to say.

The latent time of sensation, as we have thus far considered it, is the latent time for a sensation particularised in outline as to its general quality. If the quality is not to be specified, the time is shorter; if it is to be minutely specified, the time is longer. The time in the experiments just described was occupied in detecting a colour: red, blue, &c. If it were merely required to determine the presence of any light, regardless of colour, the time would be shorter; for it has been observed that when the time was too short for seeing any colour, yet an impression of light can be detected. The time required for an indefinite sensation of light of some unknown kind to arise can be put at about 0.5σ. In the interval between this point where light is seen indefinitely without recognition of its colour and the point where the colour is correctly recognised, the sensation changes in colour. A green light which is exposed long enough to be seen as light, but not long enough to be recognised as green, will appear grey, blue-grey, or blue.

For the demonstration of latent times a rapid photographic shutter can be used; it is fitted with electric contacts so that its time can be recorded. The particular arrangement that I use consists of a shutter having an adjustable opening and a closed contact. The shutter is placed before any source of light, *e.g.*, the lens of a projection-lantern whereby the phenomenon can be made visible to a whole audience. The size of the opening is adjusted by moving the slide; the rapidity

of the shutter can be increased by a rubber band. The time of exposure is first made so small that the colour is not recognised; it is then successively increased. When it has been recognised, the wires from a spark coil with condenser are inserted into two binding posts;

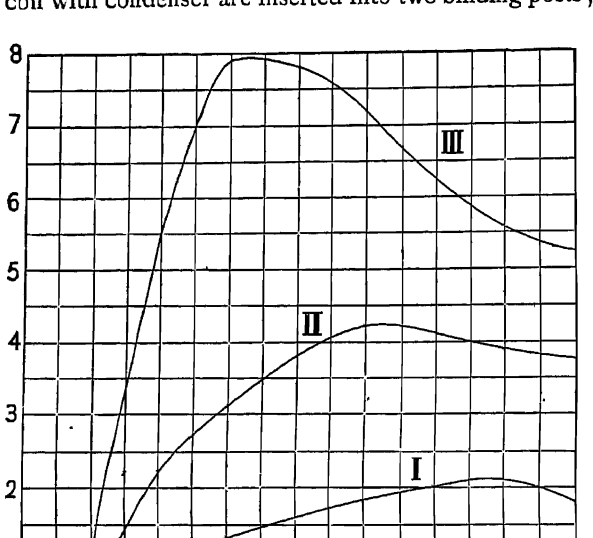


Fig. 9. ENTRANCE OF A SENSATION INTO CONSCIOUSNESS.

the current passes through the contact. Two projections on the shutter break this contact as they pass by; thus a spark is made on the drum at the beginning and at the end of the exposure. This is read off in thousandths of a second as described in the preceding chapter.

Proceeding from the consideration of the time lost

before the sensation appears at all, we inquire how long a time is required for it to reach its maximum.

The typical course of a sensation, as experimentally determined,[1] is shown in Fig. 9. There is first a short latent time, then a rapid rise to the maximum, and finally a falling off; the curves for three different intensities are given. The figures on the horizontal axis indicate thousandths of a second; those on the vertical axis are relative degrees of intensity.

We have now the course which a sensation takes after the stimulus is presented; we must next inquire how it acts when the stimulus is removed.

Let us approach the problem by the following experiment.

A series-wound electric motor is connected with a battery or a dynamo, a resistance box of the appropriate kind being inserted in the circuit. The speed of a motor of this kind is regulated by the amount of current sent through it; by adjusting the resistance we can have any speed we please.

On the axle of this motor an arbour is so arranged that discs of paper can be placed centrally on it and fastened. Two discs, one black and one white, slit radially in the way suggested by Maxwell, are slipped together so that, say, one half of the circle appears white and the other half black.

The motor is set in motion very slowly; the discs revolve. As the speed is gradually increased the white disc appears to leave some of its whiteness trailing over the black, making a grey instead of a black. With increased speed the greyish tinge spreads further and further over the black. Finally, it covers the whole

[1] Exner, *Ueber die zu einer Gesichtswahrnehmung nöthige Zeit*, "Sitzber. d. Wiener Akad," Abth. ii., 1868, lviii. 601.

black space; comparison with a separate black disc renders it easy to tell when this occurs.

At this moment the sensation of white has lagged behind the white of the disc sufficiently to cover the space of the black portion. A speed indicator attached

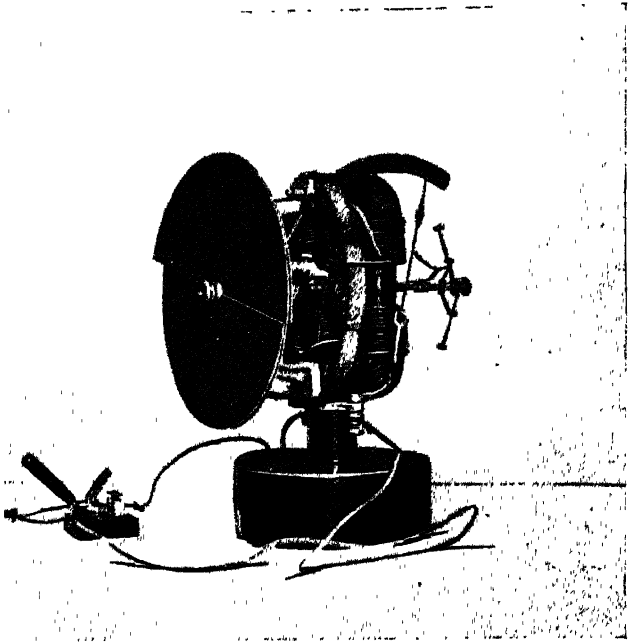


Fig. 10. ELECTRIC COLOUR WHEEL WITH SPEED INDICATOR.

to the motor[1] renders it possible to determine the number of revolutions per second made by the disc. With the disc revolving in a certain part of a second, the white-sensation must have lagged behind the white

[1] Scripture, *Some New Apparatus*, "Stud. Yale Psych. Lab.," 1895, iii. 102.

stimulus by one half the time of a white revolution, for the black surface occupied one half the time of a revolution.

This time of lag depends greatly on the intensity. For bright white paper in moderate daylight 35° would be a representative figure. The results vary, however, with the intensity. The lag of the intense sensation from a bright sun may extend even to many minutes.

It is readily noticed in the experiment just described that the sensation drops off steadily from its maximum as it spreads over the black. The portion of the black next to the white will appear a bright grey when the extreme portion is just reached by the white. By faster rotation this grey can be made to cover the whole surface. The whole disc then appears of an even grey,



Fig. 11. MIXING SENSATIONS BY RAPID REPETITION.

such as might be produced by directly mixing black and white. The white sensation can now be said to lag behind in full intensity for a sufficient time to cover the angle occupied by the black sector. Fig. 11 illustrates the way in which the white sensations are finally made to cover the black. The horizontal line indicates the time; the short lines indicate the periods of time occupied by the sectors in passing a fixed point; as the speed increases the periods are shorter. The intervals during which the white stimulus works become shorter, while the lag remains the same; finally, the interval for the black plus the time for reaching a maximum is shorter than the lag, and the whole mass of white runs together. As the white of the sector is now spread over twice as much surface, the resulting colour appears only about

half as white as the sector. The speed is read off at this point of fusion ; any increase of speed beyond that point makes no change. The results as usually stated give the time for the passage of the black sector before a fixed point as being about 25σ. This is assumed to be the amount of lag ; the true figures, however, require that the time for reaching the maximum be added to those given ; these figures, as can be seen from Fig. 9, lie between 5σ and 15σ. This makes the true time about 30σ to 40σ.

We have all along made the assumption that the black sector is a true black. This is not the case. All black objects reflect more or less light, and are therefore very dark greys. Moreover the question of intensity must not be left unanswered. The problem must be stated differently. The black must be treated as a grey of a certain measured intensity and the intensity of the white must also be given. The percentage of reflection for a white or a black surface is readily determined. The absolute intensity can be regulated by placing the source of illumination at different distances.

In the following characteristic table of results[1] the first column gives the intensity of the black surface, the second that of the white surface, and the third the time occupied by the sector (black or white) in passing by a fixed point at the moment when the fusion into grey was complete.

B	W	L
1.0	40.1	24σ
1.3	53 4	21σ
1.9	76.1	19σ
5.8	233.3	16σ
5.9	237.5	15σ

[1] Marbe, *Zur Lehre v. d. Gesichtsempfindungen*, "Phil. Stud," 1893, ix. 387.

B	W	L
11.6	465 2	13^{σ}
16 7	667.0	12^{σ}
17.2	689.6	11^{σ}
79 2	3168.1	10^{σ}

The white paper was always forty times as bright as the black. As the total illumination increased, the necessary rapidity of the rotation increased also, but far more slowly. If we are justified in considering the time occupied by the black in passing a fixed point as representing the lag, it is apparently less for the stronger light.

Thus far we have studied the latent time and the time of lag;[1] the full course of a sensation is, however, not yet determined. Let a sensation be produced by a stimulus of constant intensity; does the sensation, after reaching its maximum, remain at a constant intensity?

In the first place, it will have been noticed in Fig. 9 that the maximum is followed by a decrease which apparently leaves the sensation at a constant intensity, as long as it is present. We have, however, reason to believe that the intensity does not remain constant. It was long ago noticed that when a small black point on white paper was observed from a certain distance, the point would be visible only at intervals. Similar observations were made on very weak noises (a watch wrapped in layers of cloth), temperatures, smells, tastes, and light pressures.

The earliest measurements for sight were made for a just visible grey ring on a white ground. The complete experiment can be performed as follows: To produce a grey ground of variable intensity a white and a

[1] Experiments with tones show a latent time of 17 vibrations (*e.g.*, for a tone of 100 vibrations it is 17^{Σ}), and a lag of 6^{σ} to 40^{σ} depending on pitch and intensity.

black disc are placed together as just described. On another white disc a black circle is drawn. This disc is slipped into the others, so that as it is pulled out from behind the black it shows a piece of the ring. The set of three is now placed on the colour-wheel. When in rotation, the result is a grey surface with a grey ring on it. By adjustments of the discs the surface and the darkness of the ring can be varied from black to white.

When we look steadily at the faint grey ring, we notice that the ring appears and disappears repeatedly

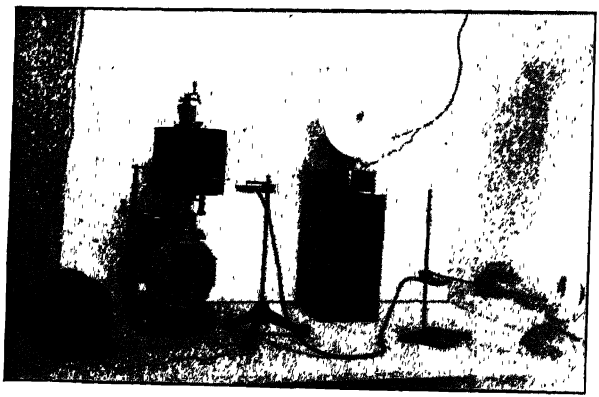


Fig 12. RECORDING THE FLUCTUATIONS OF A SENSATION.

at short intervals; our sensation from the ring appears to fluctuate from the intensity of the whole surface to a slightly less intensity.

To register these fluctuations the graphic method is again used, but the metal cylinder, or drum, for slow movements of this kind, is rotated by clockwork. A strip of paper is fastened around the drum and is then smoked in a gas flame in the usual way.

The subject takes the light wooden rod, Fig. 12, between his fingers, and as the sensation appears or

disappears he moves it up and down. This rod is connected with a rubber membrane on the top of a small capsule having a tube leading from its side; consequently every movement of the rod is followed by a movement of the air in or out of the tube. This tube leads to a similar capsule carrying a fine recording point. By placing this capsule with the point against the revolving drum a record is obtained.

A small piece of the black ring of the disc is arranged to project over the white disc. The discs are set in rotation; the surface appears of an even grey. A little more of the black ring is used; a slight shadow is perhaps noticed. Still a little more is exposed; a faint ring appears and disappears, the intervals of disappearance being long. With more of the black exposed, the

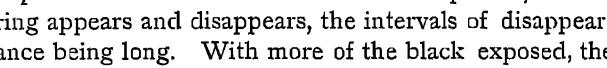


Fig. 13. RECORD OF FLUCTUATION.

ring becomes plainer and the intermissions shorter. With still more, the intermissions become less frequent. Finally the grey ring persists uninterruptedly.

For the present experiment the observer places his finger comfortably on the receiving lever. The discs are adjusted so as to appear and disappear. The observer looks at the ring; as it disappears or appears, he moves the recording lever downward or upward. This traces a record of fluctuation on the drum. Such a record is shown in Fig. 13. The time is indicated by dots produced by the pendulum contact, as described in the previous chapter.

The first measurements,[1] with somewhat simpler

[1] Lange, *Beiträge z. Theorie d. sinnl. Aufmerksamkeit*, "Phil Stud.," 1888, iv. 390.

methods than those described, showed an average period from maximum to maximum of 3.0" to 3.4" for light, 3.5" to 4.0" for sound, and 2.5" to 3.0" for electrical stimulation of the skin.

A systematic attempt [1] to determine the influence of

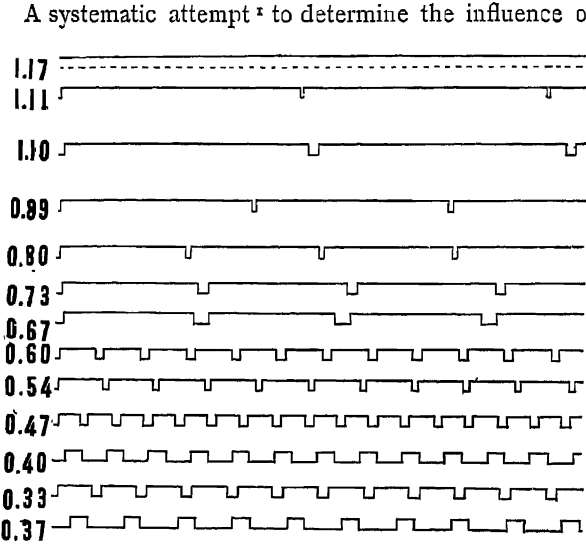


Fig. 14. INFLUENCE OF DISTINCTNESS ON FREQUENCY AND DURATION OF FLUCTUATIONS.

the darkness of the ring and the whiteness of the surface of the discs shows that there is a close dependence of the fluctuation on these factors. The general result shows that the fluctuations are most frequent with a

[1] Marbe, *Die Schwankungen der Gesichtsempfindungen*, "Phil. Stud.," 1893, viii. 615.

certain difference between the greyness of the ring and the greyness of the disc, while with a smaller or a larger difference they are less frequent. When the difference is too faint or is too distinct, there is no apparent fluctuation. As the distinctness of the ring increases the time of visibility also increases; finally it absorbs the whole time of fluctuation. These results are illustrated by Fig. 14; the figures on the left indicate the difference in intensity between the ring and the disc on a scale of 5.00 as the greatest possible difference.

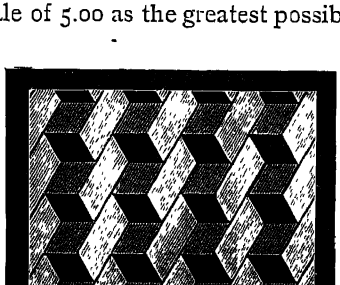


Fig. 15. THE FLUCTUATING BLOCKS.

It is noteworthy that a similar fluctuation takes place with what are called "memories." Certain sensations for which there are no corresponding processes outside the body are generally grouped for convenience under this term. If the eyes be closed, and a picture of any one of the illustrations of this book be called to memory, it will be found that the picture cannot be held in memory, but will repeatedly disappear and reappear.

Likewise if we "imagine" something, *i.e.*, voluntarily arouse sensations to which, as far as we know, no cor-

relative phenomena as determined by the other senses ever did exist, we find that the imagination disappears regularly and repeatedly.

The same is true of weak illusions. The blocks of Fig. 15 appear to have their black ends alternately above and below. The view will change against our will from one to the other as we continue to look at it.

We have now a general picture of the course of a sensation as depending on time, with its latency, its fluctuations, and its lag; let us turn to some par-

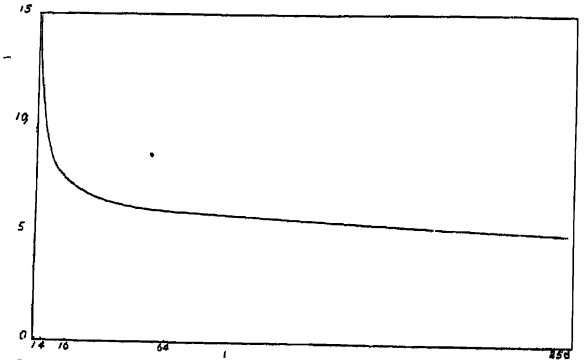


Fig. 16. SHORTEST NOTICEABLE STIMULUS AS DEPENDENT ON INTENSITY.

ticular problems. In the first place, let us study instantaneous sensations where the latent time and the lag occur in succession.

Since the sensation lags after the disappearance of the stimulus, it is possible to have the stimulus act for an interval less than the latent time and yet produce a sensation.

If the white surface on the falling block (Fig. 8) be replaced by black, the colour is shown for an instant; but instead of the colour sensation being replaced by another sensation the place is left vacant for it to persist.

In fact, the colour-sensation does persist after the paper is replaced by the black. The apparatus therefore records a time too short by the amount of lag after the stimulus has been removed. The record in this case gives the minimum time during which a stimulus needs to work in order to be noticed. We will call it the shortest noticeable stimulus.

The shortest noticeable stimulus depends upon the intensity. With lamplight of relative intensities varying from 1 to 256 there has been found to be a regular relation of time to the intensity.[1] For example, the times for blue paper thus illuminated were on one occasion as follows (Fig. 16) :

Intensity...	1	4	16	64	256
Time ...	15σ	10σ	8σ	6σ	5σ

With the apparatus of Fig. 8 experiments were made on the shortest possible exposure of various complicated objects, namely, letters and words. The letters were placed behind the grey spot, just as the colours had been. The slide fell, and the observer declared he had not distinguished the letter or else he told what it was. He attempted to name the letter even when he had seen only a part of it. Hereby he often named it correctly when he had seen only a little of it, and, on the other hand, he often thought he had recognised a letter clearly which was not present at all.

The time of exposure required for recognising letters depends on the size. With letters from Snellen's optotypes the results ran as in the following table :

D	4in	1.75	1.25	0.8	0.5
I	0.6σ	0.8σ	1.1σ	1.4σ	3.0σ
II	0.7σ	0.9σ	1.4σ	1.8σ	4.0σ

[1] Cattell, as before.

The figures for D are the distances under which the letters cover an angle of 5′ ; therefore—the fact with which we are concerned—they give the relative sizes of the letters. I and II are two different observers.

The time of exposure depends on the kind of letters.[1] The time required for recognising a letter ranges from 6σ to 50σ. Within this difference lie the various letters of the alphabet. To determine which are the most easily read, the best way is to expose each letter for a definite interval of time, and note the proportion of times it is correctly read. Among the separate letters the order of legibility depends somewhat upon the size and the font, the general relation being as follows :

Good.	Fair.	Poor.
m w d p v y j p	k f b l i g h r x t	o u a n e s c z
m w p q v y k b	d j r l o n i g h u	a t f s x z c e
d p q m y k n w	o g v x h b j l i a	t u z r s c f e

A German requires 10σ to 20σ more time to recognise a letter of his antiquated alphabet, for example, ꝛ, than to recognise a letter in the Latin type, w. But in reading words no more time is required to recognise the word in either case. The twists and tails of the old letters cause a loss of time in recognising a single letter, but in grasping the words only the main features receive attention.

"By a strange bit of perversity several of the worst

[1] Cattell, as before. Sanford, *The Relative Legibility of Small Letters*, "Am. Jour. Psych," 1888, i. 402.

letters are among those most frequently used. In a full font of type the eight letters most largely represented are e 12000, a 8500, n 8000, s 8000, t 9000, i 8000, o 8000, h 6400."

Short familiar words are just as rapidly legible as single letters. This seems to have some bearing on the modern word-method of instruction in reading.

Now that everybody reads (and some do scarcely anything else), says Sanford, it is obviously of the highest importance that reading should be made as easy and rapid as possible. If any device of paper or ink or type can shorten the time and lessen the labour even by a very little, the aggregate advantage will far outweigh the trouble, especially as saving is to be expected at the same time in the more important matter of wear and tear on the organs employed. The problem is to get the greatest amount of matter with the greatest ease of reading on the least space; or, as it has been phrased, to get the greatest legibility to the square inch. The problem has many factors, for the result depends on the tint and quality of the paper, on the ink, on the length of lines and the space between them, on the size of the letters, their proportions, the relation of their light and heavy lines, their distance from one another, and on still other details, all of which are small in themselves, but none of which can be neglected when the question is one of the maximum clearness. And all must be mutually adjusted with reference to the demands of taste and economy.

"It can be said that legibility will be favoured by enlarging the size, and increasing the differences of the letters. And it is easy to show also that legibility is favoured by simplicity of outline and concentration of the differentiations upon one particular. The influence of size is clear from the composition of the left-hand groups in the alphabets of the table, where it also appears

that breadth is as great an advantage as length. With most of the letters breadth is more of an advantage, other things being equal, than length, for it gives some visibility to their internal spaces ; short broad letters are preferable to long and narrow ones. The differences necessary to legibility have been neglected by the makers of phonetic alphabets, in their desire to indicate similarity of form. If such alphabets are ever to come into general use they will certainly have to be improved in this respect. Simplicity of outline, or what is the same thing, solid areas of black and white, will be found in most of the letters of the left-hand groups. It may even compensate for small size, as is shown by the legibility of v. In accordance with this principle, the seriphs, or little finishing strokes of the letters, for example at the top and bottom of n and the ends of s and z, should be made short and rather triangular than linear in shape. They are really more important in protecting the tips of the letters from the rounding effects of irradiation than in giving it a finished appearance, and should therefore be as small as possible and yet accomplish that object. When they are too long (as is certainly the case with the smaller letters) they easily lend themselves to confusion. The concentration of the differentiation is well seen in the groups b d p q, where each of the letters is made of a straight stem and a loop, the whole difference being made in combining the two. All are very legible letters except b, which suffers from confusibility with h. An example of lack of concentration is found in g and a, which have few points in common with other letters and yet are mistaken for many different ones."

"The element of size cannot be used to improve the relatively poor letters without at the same time shocking public taste, and opening the way for new confusions. It

is therefore from simplification and emphasis of the points of difference that help is to be expected. In the c e o group, for example, the point of distinction of c and e from o is the gap in the side."[1] It is proposed to return to the more open forms of the earlier type founders.[2] Two forms are suggested by Javal for e; one with the cross line near the top, and one in which the cross line is made longer and more prominent by an oblique position, thus ɐ. It would appear, according to Sanford, that the first of these is about as confusable with c as the common form is. The two forms of the Greek epsilon, ε and ϵ, suggest themselves as possible substitutes.

"Another group of the poor letters includes a, n, and u. The distinction of n and u from each other and from a ought to be helped by keeping their openings at the top and bottom as open as possible. Javal points out the curved top of the a as a point of resemblance to n, and recommends a form of the first letter found in the Italian manuscripts that furnished the model for some of the early typemakers. In this the top is very small and the loop is relatively long horizontally, giving the letter the appearance at a distance of an inverted r : ɹ. Even in the less exaggerated form which the letter would be given if adopted, it could easily be distinguished from u and n, and from s also, with which it has some tendency to confusion. The great legibility of v suggested that its inverted form, the small capital ʌ, might be substituted (after the analogy of c, o, s, v, w, x, and z) for the present a. There is a slight advantage for it in spite of the handicap of the added letters."[3]

Cattell says that s is "hard to see," and the number

[1] Sanford, as before.
[2] Javal, "Rev. Scientifique," 1881, xxvii. 802.
[3] Sanford, as before.

of times no answer at all was ventured for it, together with the wide scattering of its confusions, show him to be right. Javal, too, thinks it rather a hopeless case, but suggests the sharpening of its angles as a way of making it approach the legibility of z.

Turning to another application of the experiments in the time of sensation, we find that the fact of the lag

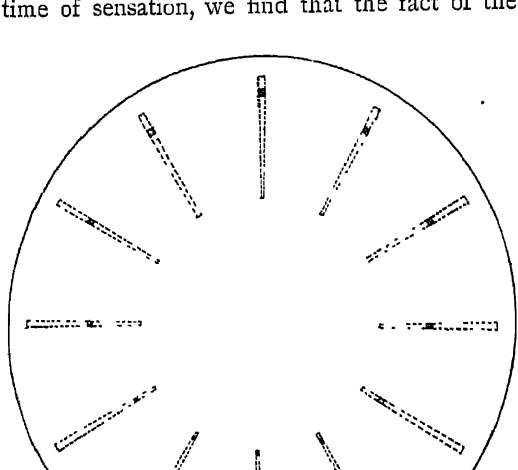


Fig. 17. THE STROBOSCOPE.

of the sensation is used in making the change from one picture to another to be apparently without break, whereby a series of slightly different pictures presented rapidly in succession is made to appear as a continuous object in motion. This is the principle of the stroboscope.

The fundamental law of the stroboscope can be

illustrated by treating the movements of a dot. Two discs are prepared as in Fig. 17. The rear disc contains a series of twelve dots at different distances from the centre. The disc is rotated by clockwork. The front disc contains twelve equidistant slits. The eye is placed before one of the slits. As the front disc is rotated, the eye catches successive glimpses of the rear disc. A screen allows the eye to see only one of the slits at a time.

We will suppose that both discs are rotated in the same direction at the same rate. As one slit passes the eye a glimpse is caught of the opposite dot, say at its position further from the centre. The view is then cut off till the next slit appears. The eye catches the dot now opposite, but this time the dot is a little nearer the centre. When the next slit comes round, the dot appears to be still nearer. And so on. The dots are really different dots, but they might just as well be the same dot in different positions.

Now, when the rotation is swift enough, the interval between the dots is filled by the lag of the sensation. Imagination does the rest. Instead of one dot in a succession of positions, the dot seems to move steadily through the whole distance covered by its positions. With a disc like that in the figure the dot seems to swing back and forth like a pendulum.

This form of the stroboscope where the figures and the slits revolve in the same direction has been called the phænakistoscope. When the figures and the slits go in opposite directions, a continuous movement is likewise obtained. This form of the stroboscope is called the dædalum. As the two names phænakistoscope and dædalum are exceedingly awkward, I venture to propose the terms direct stroboscope and reverse stroboscope.

The length of exposure of each dot (which is determined by the width of the slit), the time between the exposures (which depends on the distance between the slits), the number of exposures per second (which depends on the rate of rotation and the difference between two successive pictures), are all prime factors of the illusion of rotation. The minimum requirements for this illusion are governed by various psychological laws. Strangely enough, only one investigation has been made;[1] I will state the results briefly.

The time during which the dot is visible for the same rates of rotation is longer with the direct stroboscope than with the reverse stroboscope. Since each dot is moving before the eye, it will tend to make a blurred image with long exposure; the reverse stroboscope therefore gives sharper images. In obtaining the following results the reverse stroboscope was used exclusively.

With a very slow rate of rotation the eye sees a dot in a succession of different positions. As the rate is increased, a point is reached where the dot appears in continuous motion. This point we will call the ascending threshold of fusion. If, on the contrary, the dot is in continuous motion and the rate is gradually decreased, a point is reached where the motion breaks up into a succession of positions. This is the descending threshold. The ascending threshold is always higher than the descending threshold. This is an illustration of one of the universal facts of mental life, namely, the opposition to change or, we might say, the inertia of mind. Having seen the dot in a succession of positions, we continue to regard it as in such a succession even with rates of rotation which would otherwise cause us to regard it as in continuous motion. On

[1] Fischer, *Psychologische Analyse der stroboskopischen Erscheinungen*, "Phil. Stud.," 1886, iii. 128.

the other hand, having seen it in continuous motion, we persist in so regarding it even with rates slow enough to ordinarily produce a succession of positions.

Up to this point the eye has been supposed to be seeing only one dot through the screen; by exposing two or more slits the eye sees two or more dots passing through successions of position, or in continuous motion. If the threshold be now determined, it will be found that it steadily rises as more dots are exposed. That is, as the pictures become complicated, the number of exposures per second must be increased in order that all points shall be seen in continuous motion.

In regard to the time of exposure the law is that the threshold of repetition, *i.e.*, the minimum number of repetitions necessary for continuous motion, is inversely proportional to the time of exposure. The time of exposure can be varied by changing the width of the slits.

This after-impression, when weak, is a rather indefinite thing in any case, so a little suggestion is sufficient to make us believe that this image which filled up the time between two dots actually moved slightly. The power of this suggestion is seen in the difference between the ascending and descending results just mentioned. It is very marked in the following case: A card is printed with pictures of two children asleep. By raising the card so that the light falls through from the other side, the children open their eyes apparently by lifting the lids. Of course the eyes were printed on the back. The movement was plain, yet the transition was really a sudden one, from shut to open.

Any complicated object can be considered as made up of an infinite number of points. A set of stroboscopic images will, under the appropriate conditions, be fused to a single object in movement. Flat pictures can be

considered as made up of a number of points. If a rapid succession of pictures of a moving object be taken, they can, under the proper conditions, be combined by the stroboscope into a single object of motion.

The reverse stroboscope in a cylindrical form (Fig. 18) is convenient for the presentation of a succession of photographic pictures of objects in motion. In the form prepared by Anschütz the moving object is photographed by a series of cameras. The series of pictures is then printed on a strip of paper, which is bent around and placed on the inner surface of the cylindrical box-cover. The slits are cut directly in the strip. The cover is set in rotation around its centre, and the eye views the pictures through the slits.

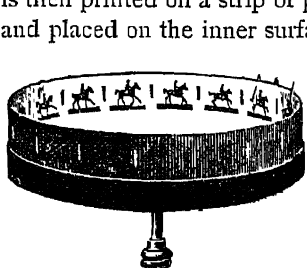


Fig. 18. CYLINDRICAL STROBOSCOPE.

A great impulse to the application of this method of studying motion was given by Marey's development of a single camera for repeated exposures.[1] The variations of the method are many; I select that of Edison for illustration.[2]

Pictures are made on a band of celluloid one and a half inches wide, the additional width over one inch being required for the perforations on the outer edge.

[1] Marey, "La methode graphique," "La photochronographie," and other works, especially the popular exposition, "Le mouvement," Paris, 1894.

[2] The following account of the kinetoscope pictures and the kinetoscope is derived partly from Dickson, "Life of Edison," and partly (including Figs 20 and 21) from "La Nature," Oct. 20, 1894, by way of the "Literary Digest," 1894, x. 105.

These perforations occur at close and regular intervals, in order to enable the teeth of a locking device to hold the film steady for nine-tenths of the one forty-sixth part of a second, when a shutter opens rapidly and the film catches a phase in the movement of the subject. The film is then jerked forward in the remaining one-tenth of the one forty-sixth part of a second, and held at rest, while the shutter, having made its round, again exposes the film, and so on. Forty-six impressions are taken a second, or 2,760 a minute. This speed yields 165,600 pictures in an hour.

In this connection it is interesting to note that were the spasmodic motions added up by themselves, exclusive of arrests, on the same principle that a train record is computed, independent of stoppages, the incredible speed of twenty-six miles an hour would be shown.

The next step after making the negative band is to form a positive or finished series of reproductions from the negative. The negative is passed through a machine for the purpose, in conjunction with a blank strip of film which, after development and general treatment, is placed in the kinetoscope. In Fig. 19 I give a portion of such a strip of pictures (kindly furnished by Mr. Edison).

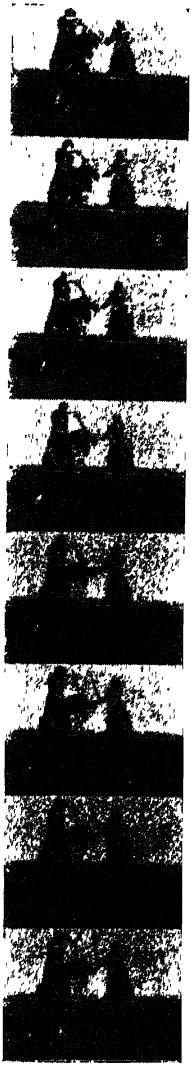


Fig. 19. PIECE OF KINETOSCOPE RIBBON

The taking of the pictures is variously performed—by artificial light in the photographic department, or by daylight under the improved conditions of the new kinetoscope theatre. The actors, when more than one in number, are kept as close together as possible, and exposed either to the glare of the sun, to the blinding light of four parabolic magnesium lamps, or to the light of twenty arc lamps, provided with highly actinic carbons, supplied with powerful reflectors equal to about fifty thousand candle power. This radiance is concentrated upon the performers while the kinetograph and phonograph are hard at work storing up records and impressions for future reproductions.

When the ribbon with the succession of pictures has been prepared, it is presented to the eye by the kinetoscope.

A popular and inexpensive adaptation of the kinetoscope is in the form of a machine, consisting of a cabinet containing an electrical motor and batteries for operating the mechanism which acts as the impelling power to the film. The film is in the shape of an endless band from fifty to one hundred and fifty feet in length, which is passed through the field of a magnifying glass perpendicularly placed. The photographic impressions pass before the eye at the rate of forty-six per second, through the medium of a rotating, slotted disc, the slot exposing a picture at each revolution, and separating the fractional graduations of pose.

The kinetoscope is shown in Fig. 20. It is enclosed in a wooden box furnished on top with a lens. The eyes being placed at this lens, one sees a transparent picture.

How is this apparatus operated? Turning to Fig. 21, in which the mechanism is exhibited, it will be seen that there are two compartments, one above the other. The mechanism is contained in half the depth of the case,

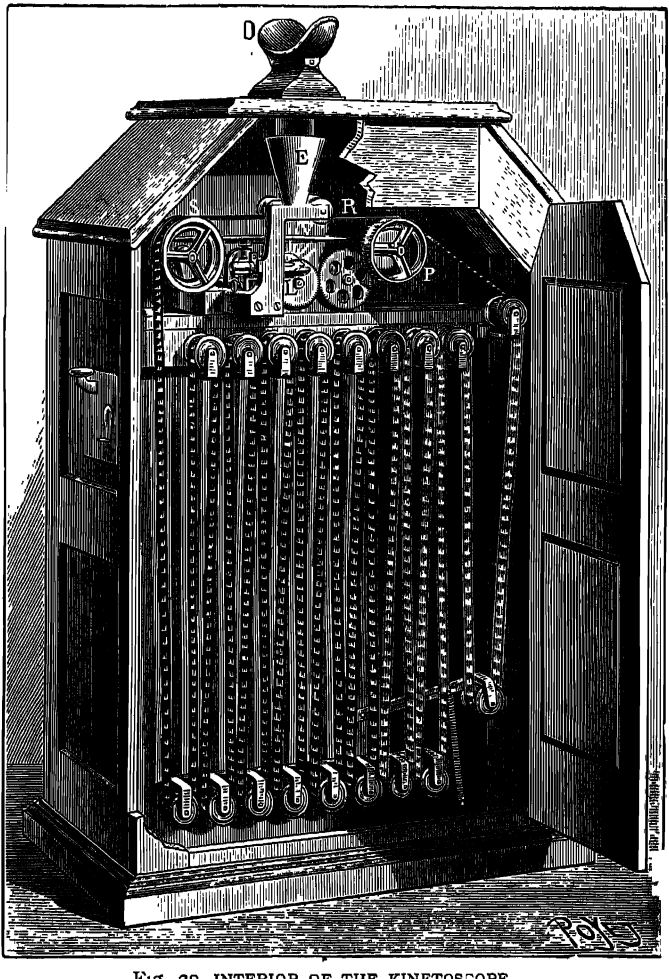


Fig. 20. INTERIOR OF THE KINETOSCOPE.

the other half being reserved for the ribbon of pictures, as shown in Fig. 20.

At the foot of Fig. 21, in the lower compartment, is shown the electric motor C, which sets all the mechanism in movement. It is an Edison dynamo of eight volts, operated by four accumulators, with a capacity of eighty ampere-hours. The current passes across a resistance, which is varied to augment or diminish the light of the incandescent lamp. This renders the ribbon of celluloid more or less transparent, according to its thickness and transparency, which are very variable.

In the upper compartments of Fig. 21 there is a metal disc V, which forms a screen before the ribbon R. The little incandescent lamp, which lights up the ribbon and renders it transparent, is shown at L. The lens O, where the observer places his eyes, is mounted on a conical tube E. When one wants to operate the apparatus, the electric motor is set in motion. By means of a mechanism of toothed wheels the motor turns the circular metal disc V; this is furnished with a slit F, which enables the observer to see the pictures on the ribbon at R every time the slit passes over the lamp. The ribbon is above the metallic disc. It glides over the pulleys P S.

If the ribbon of the kinetoscope contained a series of dots each in a slightly different position, the result would appear to be a dot moving through all these positions. Pictures are combinations of infinite numbers of dots each in a succession of positions; each of these dots repeats the movement of the object from which it was taken. Consequently the succession of pictures of the kinetoscope reproduces all the movements of the original scene.

When a phonographic record has been taken simultaneously with such a strip, the two are started together

by the use of a simple but effective device, and kept so all through, the phonographic record being in perfect accord with the strip. In this conjunction the tiny holes with which the edge of the celluloid film is perforated

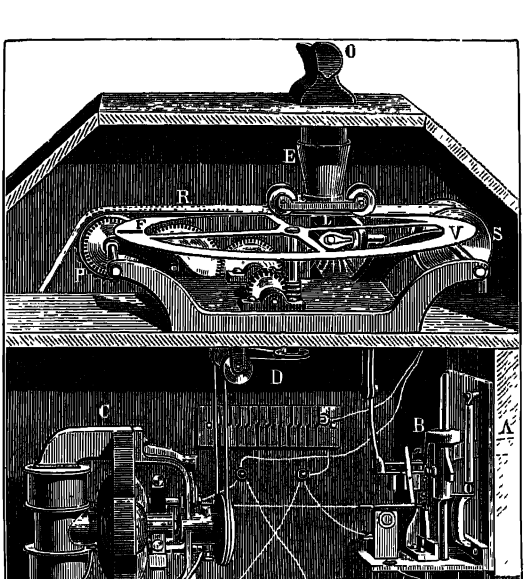


Fig. 21. DETAILS OF UPPER PART OF THE KINETOSCOPE.

correspond exactly with the phonographic record, and the several devices of the camera, such as the shifting of the film and the operations of the shutter, are so regulated as to keep pace with the indentation made by the stylus upon the phonographic wax cylinder, one motion

serving as a source of common energy to camera and pho ıograph when they are electrically and mechanically linked together.

The last step in kinetoscopic pictures from life is the

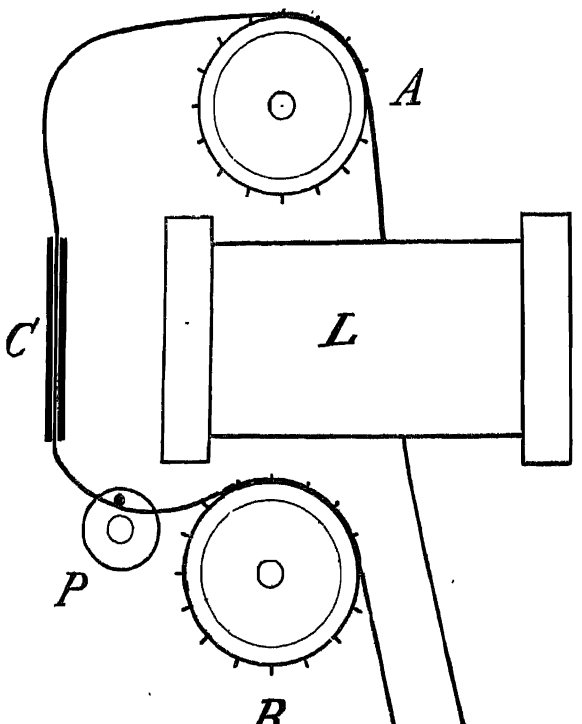


Fig. 22. MECHANISM OF THE VITASCOPE.
A, B, Feeding wheels; *C,* Carrier plates, *P,* Eccentric; *L,* Lens of lantern.

adaptation of them to projection on the screen. In Edison's vitascope, a strip of pictures similar to those of the kinetoscope passes through a lantern in such a way as to take the place of the ordinary lantern slide. The

film runs over a wheel having teeth at the sides which pull the film along. It then passes between two plates with an opening about an inch square, corresponding to the slide carrier of the lantern. An electric light illuminates the film at this opening and a lens in front projects it on the screen. If the film were in constant motion nothing but a blur would be seen. To avoid this the film is made to move forward by sudden jerks. This is accomplished by an eccentric pin which strikes the celluloid film once every revolution. The metal plates between which the film passes are lined with cloth, which keeps it from moving unless pulled. It unwinds from the upper wheel but does not move past the opening; this is the period during which the picture is thrown on the screen. The eccentric now strikes it below the plates and pulls it forward with a certain jerk so that the next picture appears on the screen. This is repeated forty times a second. During the greater part of this fortieth of a second, the film is at rest behind the lens; the time occupied by the eccentric in jerking the film forward is so minute that the movement is invisible for the eye. Consequently a series of slightly different pictures is produced in rapid succession on the screen and these fuse into a single picture of objects in motion. Such views as a street scene with moving cars and people, or the rescuing of persons from a burning building are rendered so effectively that they appear perfectly natural.[1]

To complete the lantern method by a better representation of life, the pictures may be coloured. There is still lacking a method for producing the effect of depth, so that the figures shall not remain on the screen but

[1] The machines of Lumière, Demeny and others are presumably constructed on the same principles as the vitascope.

apparently move toward or away from the observer; the method of producing the appearance of depth with ordinary slides will be described in the chapter on binocular vision; it has not yet been adapted to the vitascope.

CHAPTER VII.

TIME OF VOLITION.

In addition to sensations we experience various activities, which we commonly call voluntary acts or volitions. Can the time required for willing and executing an act be measured? We will approach the problem by considering the rapidity with which an act of will can be repeated.

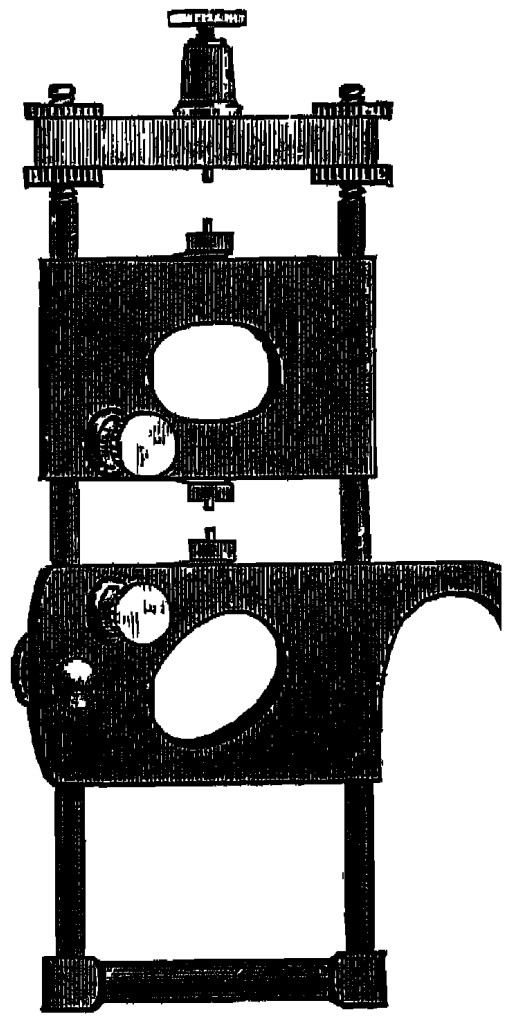


Fig. 23. REACTION KEY

With a method of measuring time to the 1–1000 sec., established as described in Chapter VI., it remains only to find a means of recording an act at the moment it occurs.

For studying the movements of the forefinger, a special electric key, called the reaction key, is used (Fig. 23). It consists of two hard rubber slides running on steel guides.[1] The upper slide has a hole to fit the end of the finger. The other has an inclined hole for the thumb. The thumb may be placed against the

[1] Scripture and Moore, *A New Reaction Key, and the Time of Voluntary Movement*, "Stud. Yale Psych. Lab.," 1893, i. 88.

projecting arm; this arrangement gives a somewhat easier action, as the finger moves more naturally in a plane inclined to that passing through thumb and finger. The lower slide is fastened at any point by a clamp whose screw is seen to the left in the figure. This determines the range of movement of the upper slide.

The key is placed in the primary circuit of the spark-coil, and the secondary circuit is sent through the drum and fork as in Fig. 6. The key thus takes the place of the contact (Fig. 5) and makes a spark record on the time line at each extremity of its movement. The fork is assumed to be perfectly correct, and the time between each pair of sparks is counted in 1000ths of a second. With movements repeated as rapidly as possible results have been obtained of the following character[1]:

Distance of Movement.	Time of Extension.	Time of Flexion.	Complete Time.
5 mm.	33^{σ}	48^{σ}	81^{σ}
10 mm.	40^{σ}	48^{σ}	88^{σ}
20 mm.	53^{σ}	37^{σ}	90^{σ}

The extent of the movement makes very little difference; we can accept the time for the distance of 5 mm. as nearly the time for alternating voluntary acts, and can take 40^{σ} each as a close approximation to the time for willing and executing acts in rapid succession.

The objection may be raised that the two acts of will overlap.

If by "act of will" is meant the memory-idea of the movement to be executed (followed by an act), they not only overlap, but are both simultaneously present throughout the experiment. This is not what is meant

[1] Scripture and Moore, as before, p. 90.

here by voluntary act. We may have vividly pictured to ourselves by sight or by feeling the act to be performed, and yet the act does not follow until we *will* it to do so. The *act* that follows the idea of the movement is what we know as the voluntary act, or the volition. We need not enter into any discussion concerning what "the will" is, any more than concerning what "sensation" is. We all experience sensations and we all experience volitions. It is our business here to determine the laws according to which they occur.

Two volitions of opposite character are necessarily successive; we cannot will to walk backwards and forwards at the same time. We can therefore state the time of volition, under the particular circumstances of the experiment, to be about 40σ.

Does a volition have a latent time and a lag similar to a sensation? In the experiments just referred to, the electric current was sufficiently powerful, not only to make a large spark at the moment the movable slide broke the circuit by leaving either extreme, but also to make a fainter one when the slide first struck the contact at either extreme. Thus there was a record of the time during which the slide rested at either extreme before changing to the opposite motion. This time of rest, or latent time and lag together, was quite different at the two extremes. At the end of the extensor movement it was general 2σ or 3σ, occasionally 4σ. At the end of the flexor movement it was so short that the two sparks could seldom be separated; the time was thus in general less than 1σ. Both latent time and lag were presumably present, although they could not be separated. Apparently both are smaller than in the case of sensations.

The course of a volition can therefore be represented as similar to that of a sensation with its latent time and

its lag. It also undergoes fluctuations similar to those of a sensation.

To illustrate the fluctuation of a volition the subject may be required to press his thumb and finger together with

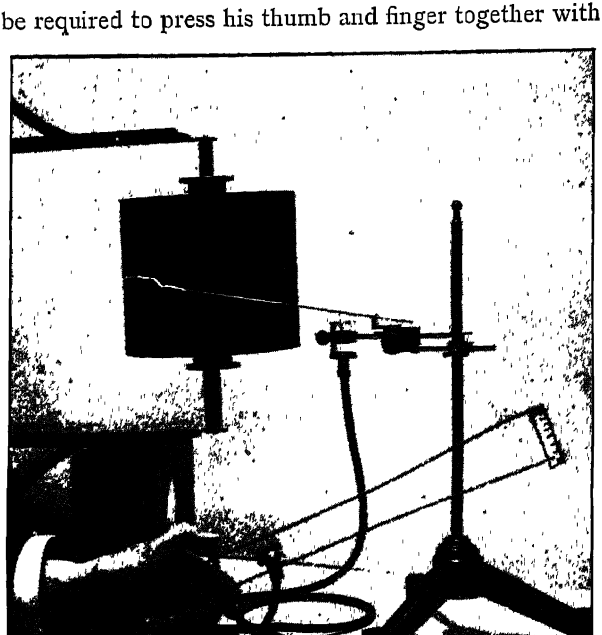


Fig 24. RECORDING FLUCTUATIONS OF A VOLITION.

a certain force. The original force to be exerted is left to his own choice, but when chosen it must be maintained without change. The apparatus for the experiment is similar to that shown in Fig. 4. In order to take graphic records, a glass cylinder with a gutta-percha

piston is added to the dynamometer. The rubber tube for the cylinder runs to a piston-recorder, so constructed that the movements of the piston are amplified by a light recording arm, resting against a smoked drum.

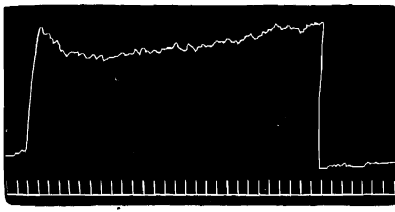


Fig. 25 FLUCTUATIONS IN A VOLITION INTENDED TO BE CONSTANT.

The whole arrangement, called a dynamograph, is shown in Fig. 24. A record of what the subject supposes to be a constant effort will be similar to that shown in Fig. 25. The checks on the line at the bottom indicate seconds. There seem to be minor fluctuations of about $\frac{3}{4}$ second intervals, with a large fluctuation downward and a recovery. A similar fluctuation is found when the subject makes repeated instantaneous grips of what he considers to be equal strength (Fig. 26).

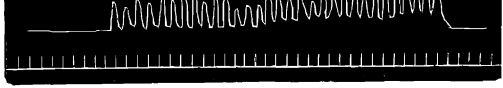


Fig. 26. FLUCTUATIONS IN THE STRENGTH OF A REPEATED VOLITION.

Having thus obtained a picture of the general course of a volition, let us look at some particular cases.

Rapid alternation of voluntary movements forms the basis of many activities. The activity most extensively

studied is that of finger movements. One form of this activity, that of tapping a telegraph-key, has been extensively used in investigating mental life.

A telegraph-key (Fig. 27) is inserted in the spark-coil

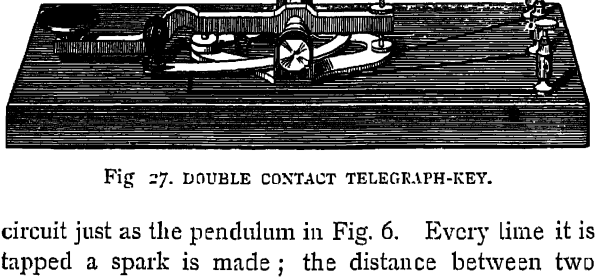


Fig 27. DOUBLE CONTACT TELEGRAPH-KEY.

circuit just as the pendulum in Fig. 6. Every time it is tapped a spark is made; the distance between two sparks represents the time of one complete tap, and is called the tap-time. The tap-time thus includes both the downward movement and the recovery. Of course, the shorter the tap-time, the greater the rapidity of tapping.[1]

We have already noticed that the mean variation is as characteristic a quantity in mental life as the average itself. We shall have occasion to notice changes in the regularity of the tapping. It is to be remembered that the various facts we shall now note relate to the

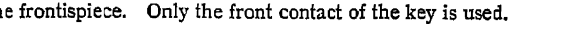


Fig. 28. FLUCTUATIONS IN THE TAP-TIME.

changes in the maximum rapidity with which volitions can be alternated.

The average tap-time, when the tapping is as rapid as

[1] The experimental arrangements are the same as those shown in the frontispiece. Only the front contact of the key is used.

possible, undergoes a fluctuation,[1] as can be seen in Fig. 28, where the horizontal line indicates 150σ, and the points above and below it indicate the tap-times in a succession of taps.

The mental condition has a most powerful influence on the rapidity of tapping. Excitement makes the tapping more rapid. The influence of distraction of attention is shown in Fig. 29; the figure has the same meaning as Fig. 28. Adding 214 and 23 produced a marked lengthening in the tap-time; so did the mental labour of multiplying 14 by 5. It takes some effort for an ordinary man to perform these calcu-

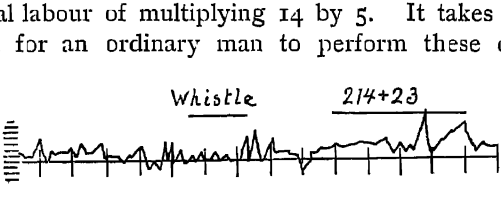


Fig. 29. DEPENDENCE OF TAP-TIME ON ATTENTION.

lations, and the mental work of association seemed to leave less energy for the work of volition. The thought suggests itself of the possibility of measuring the amount of work involved in various school exercises by the influence on tapping. Fig. 29 seems to show also that momentary distractions not involving any mental work, such as disturbance by some other person through clicking the tongue, or lighting a match, do not change the rapidity. They do, however, improve the regularity; the curve is smoother. It is a note-

[1] Bliss, *Investigations in Reaction-Time and Attention*, "Stud. Yale Psych. Lab.," 1893, i. 45.

worthy fact in all our mental life that the less attention we pay to an act, the more regular it is.

When it is desired to determine how many taps can be made in a minute, it is sufficient to connect the key with a magnetic counter, in which a pointer moves over a dial one unit for every tap. The number of taps that can be made in ten seconds in one condition of mind, is compared with the number that can be made in another condition.

Strong mental excitement is favourable to rapidity of tapping. Records taken immediately before sorting into heaps as rapidly as possible eighty cards of ten different kinds, show, when compared to the records taken immediately afterwards, a lessened time for the latter.[1] After studying the variations caused by such mental work, and taking into account that vigorous physical exercise had produced the opposite effect, it seemed probable that the increase in rate was due to increased mental activity and the unconscious tension of the muscles attending this mental excitation. This view was somewhat strengthened, when a study of the whole series of experiments revealed the fact that an increased rate had accompanied other mental excitements. For example, the rate was increased after reading an interesting, unexpected letter, after the announcement of a distinguished visitor to inspect the work, and just before reading a paper before a class.

Fatigue tends to increase the tap-time, and to make it more irregular. With the key shown in Fig. 23 experiments have been made, wherein the subject kept tapping continuously for a very long time. Fig. 30 shows the manner in which the changes occur.[2]

[1] Dresslar, *Some Influences which Affect the Rate of Voluntary Movements*, "Am Jour. Psych.," 1891, iv. 514.

[2] Moore, *Studies of Fatigue*, "Stud. Yale Psych., Lab.," 1895, iii. 92.

A study of fatigue in tapping, by Bryan,[1] brought out the following facts: fatigue begins to show itself by a perceptible lowering of the rate after 10 or 15 seconds of work; after 10 to 15 minutes the reduction is considerable; thereafter the reduction goes on slowly. Partial recovery takes place very quickly, but complete recovery very slowly; tapping from one joint induces fatigue for that joint, but does not noticeably affect remote joints.

The rapidity of tapping is different for the different

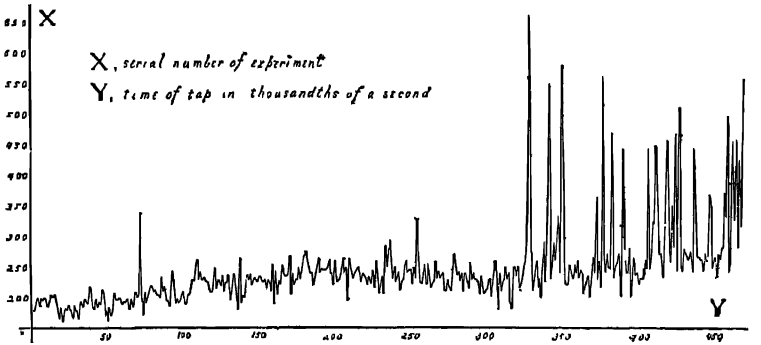


Fig. 30. INFLUENCE OF FATIGUE ON TAP-TIME.

joints of the arm. The general sequence in order of rapidity is elbow, wrist, shoulder, finger. A joint on the right side is generally more rapid than the corresponding one of the left side. The superiority of the boy's right side over the girl's right side is slightly greater than the superiority of the boy's left side over the girl's left side.[2]

The rapidity of tapping varies with age. Bryan has determined the progressive development for the various

[1] Bryan, *On the Development of Voluntary Motor Ability*, "Am. Jour. Psych.," 1892, v. 123.

[2] Ibid.

joints during the different years of school life. In his experiments the highest rate out of 46 children was attained by a girl of 12 years, whose record exceeded that of the average of the age by 30 % to 40 %. She looked the type of robust health. When asked if she played the piano, she said: "Only by ear, but I play base ball, though;" adding, a moment later, "I can strike two over the octave on the piano." Another girl, 13 years, who had taken lessons on the violin for two years, gave records almost as much above the average as the girl just mentioned, except for the left elbow, and especially for the left shoulder. The high rates of the joints most involved in playing the violin and the low rate of the left shoulder illustrate the effect of practice.

The results of measurements of tapping-time on one hundred New Haven school children of each age from six to seventeen are shown in Fig. 31. The figures at the left give the number of taps in five seconds, those at the bottom the ages. The little children are very slow; the boys at each age tap much faster than the girls.[1]

In these experiments the children continued tapping after the five seconds recorded. After tapping thirty-five seconds longer a record was again taken. The difference between the two sets of records tells how much the child lost owing to fatigue. At six years of age, the boys lost 23 % of the original number of taps. The amount of fatigue was greatest at eight years, and decreased with advancing age. It is very remarkable that, without exception of a single age, the girls were less fatigued than the boys. A comparison of the results suggests a conclusion as to the impetuosity of the boyish character.

[1] Gilbert, *Researches on the Mental and Physical Development of School Children*, "Stud. Yale Psych Lab.," 1894, ii. 63.

Such work on fatigue suggests the possibility of an experimental study of the pathological cases of fatigue

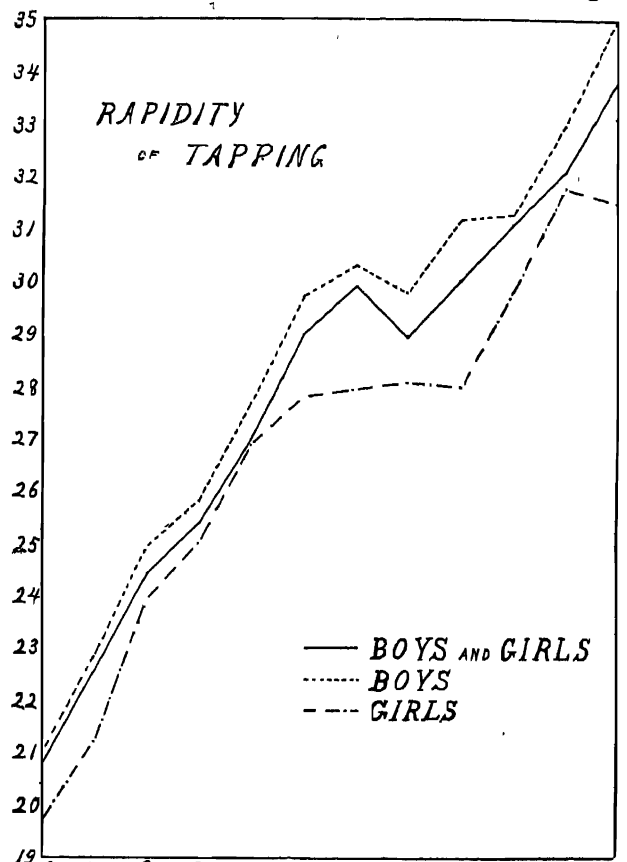


Fig. 31. RESULTS OF EXPERIMENTS ON NEW HAVEN SCHOOL CHILDREN.

in tapping that appear among telegraphers, violinists, piano players, &c. Might it not also be possible by

preliminary experiments on fatigue to detect and warn would-be students of telegraphy or type writing, who are in danger of being suddenly incapacitated in their chosen trade? The seriousness of losing a means of livelihood attained after costly training would justify an attempt to detect the weakness beforehand.

According to Dresslar the rapidity of tapping varies with the time of day. The average of six weeks of work gave the following results: at 8 a.m. the time required for making 300 taps was 37.8^s; at 10 a.m., 35.5^s; at 12 a.m., 34.6^s; at 2 p.m., 35.5^s; at 4 p.m., 33.5^s; at 6 p.m., 35.1^s. It is noticeable that these results corresponded to the habits of the previous two years of the person experimented upon; these years were spent in public school work with a daily programme, beginning at 8 a.m. and closing at 4 p.m., with an hour and a half intermission at noon.

The rapidity of alternating volitions has been studied in several particular forms. It has been thought worth while to devise an apparatus for studying the rapidity of a boxer's blows. It consists of a small stiff cushion, against which the fist is struck: an electric contact is made at each blow. The rapidity with which such blows can be struck is a matter of some interest. One curious fact has been noticed: the number of blows per second, for both hands striking in alternation, is only about 30 % greater, instead of twice as great as the number for one hand singly. It seems to be in some way a question of the difficulty of willing two things rapidly in succession.

A special form of this problem has been investigated in the case of producing trills, *i.e.*, alternations of two movements on the piano.[1]

[1] Binet and Courtier, in "Rev. Scientifique"; reported in "Nature" 1895, l. 11597, from which I quote the account. The account also appears in "L'Année psychologique," 1895, ii. 201.

The apparatus consists chiefly of an indiarubber tube, placed under the key-board, united at its extremities to a registering capsule. When the notes of the piano are played, the pressure on the tube causes waves of air to be sent through it into the capsule, whereby a record may be made on the drum as in Fig. 24. The board on which the tube rests is regulated by means of wedges adjusted by a screw, the board being either lowered or raised. When raised it almost reaches the keys of the piano, and in this case registering action takes place; but if it is lowered, the whole apparatus is disconnected from the key-board. Fig. 32 illustrates trills, and shows

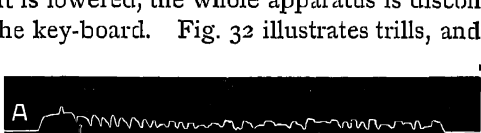


Fig. 32. RECORDS OF TRILLING ON THE PIANO: A. BY AN AMATEUR; B. BY A PROFESSIONAL.

clearly the equality or inequality of touch; the records show, too, the amount of irregularity.

It has perhaps seemed strange to consider tapping as a mental affair. When we will to press the button once and do so, the single tap was evidently an act of will. We likewise will and execute the second tap, the third, &c. We can repeat the tap faster or slower, regularly or irregularly, strongly or weakly, just as we will to do. There are certain limits that we cannot pass; we cannot in particular go beyond a certain rapidity, or attain more than a certain regularity. The actual rate is influenced by the amount of work done (space traversed or resistance overcome); more than a certain amount of work we cannot do, unless

the conditions are changed, *e.g.*, by excitement. An effort of will represents an expenditure of just so much energy, which in a careful investigation might be calculated. When it is said that with the key in Fig. 23 or Fig. 27 just so many taps were made, it is meant that just so many efforts were put forth to move a weight of so many grammes through a distance of so many millimetres and back.

It seems quite justifiable to assume that the rapidity of tapping represents the rapidity with which the impulses to do the required amount of work are repeated. I think it unquestionable that the results of distraction, excitement, &c., are due to changes in the process of willing. Possibly, also, the fatigue is to a large extent mental, if not mainly so.

Following out the same line of thought, we are driven to the conclusion that successful men of muscle must be men of mind also. Walking, running, rowing, &c., require the rapid repetition of certain muscular acts. The rapidity and regularity attainable depend upon the will. The maximum rate attainable is a fairly constant figure. The constancy of the maximum rate of motion is indicated by the small limits within which the racing records of a given individual vary. Notwithstanding the large number of motions made by a horse in running one mile, a dozen successive race records are not expected to have a gross variation of more than two or three seconds, if the horse, the track, the weather, &c., are each time in about the same conditions. The same holds true of bicycle riders, oarsmen, &c.[1]

[1] Bryan, as before.

CHAPTER VIII.

TIME OF REACTION.

By reaction we mean conscious action in response to a signal. Experimental arrangements are made for producing a signal and recording it simultaneously on the drum, and for recording a movement in response. This can be done readily and accurately by use of a touch-key and the telegraph-key.

The touch-key, Fig. 33, is so arranged that at the moment the rubber button at its end touches the skin

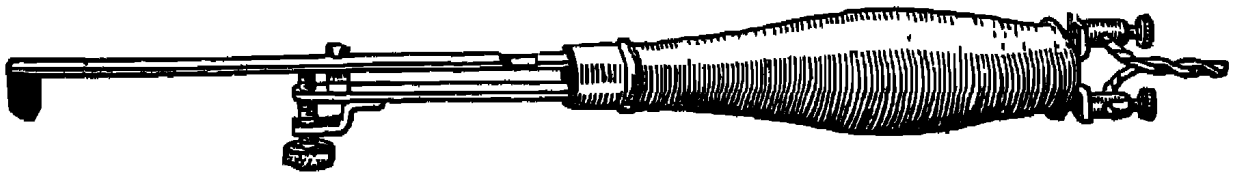


Fig. 33. TOUCH-KEY.

it breaks an electric circuit ; it closes the circuit again instantly, so that the current is ready for interruption by the telegraph-key. This telegraph-key is shown in Fig. 27. The finger is placed on the knob ; the current is sent through back contact. Both keys are placed in the primary circuit of the spark coil just as the pendulum contact was for the 100 fork (Fig. 6). Any movement of either key breaks the circuit and makes a spark ; consequently there is one spark on the time-line for the touch and one for the responding movement. The number of thousandths of a second is easily read off.

The instructions to the subject are : close your eyes and get ready to press the key *instantly* at the moment you are touched. The subject knows the movement that is to be made and prepares himself so that the act of will follows without any consideration of what is to be done. It follows at the moment the sensation is strong enough to be noticed. The time marked off between the two sparks thus includes the time required for the sensation to arise, and the time of the voluntary act. With practice and strict attention to the desired conditions, the total time can be reduced to about 100σ.

For accurate work, however, the subject must be absolutely free from disturbance or distraction. This requires that he shall be alone in a perfectly quiet room, and this requirement brings the necessary consequence that the methods of experimenting shall be so modified as to permit such isolation.

My experience with the unavoidable noisiness of the quietest rooms at other universities led me to attempt the construction of an isolated room at Yale. I selected a small room in the middle of the building, where, with the exception of a door, the surroundings consisted of solid walls and floors. In this room an interior room of light wood was built. This interior room was supported by four pieces of rubber. The space between the walls of the two rooms, which everywhere exceeded six inches, was filled with sawdust. Around the doorway and the ventilator the sawdust was kept back by canvas. There was thus no solid connection between the exterior and the interior room except through the rubber. Both the outer and the inner room possessed thick doors. The quietness of the isolated room is remarkable. Very loud sounds in the adjacent rooms or overhead can still be heard through the ventilators. Heavy waggons on the street shake the whole building,

and can be felt through the feet but not heard. The main difficulty lies in the method of ventilation. A rotary fan blower in a distant part of the building forces a large volume of air through the three sound-killers. The sound-killer consists of a large box lined with felt, and containing felt diaphragms whereby the air is forced to take a zig-zag path. The sound from the blower cannot well pass through such a tortuous path, where the soft walls prevent reflection. The first sound-killer is close to the blower. From this the air is carried about fifty feet to another sound-killer just above the isolated room. As the air enters into the room it passes down to the floor through the third sound-killer.

For high grade work such an isolated room is as necessary in the psychological laboratory as the room of constant temperature in the physical laboratory or the non-shakeable base for the astronomical observatory. As the result of my own experience, I would suggest numerous important changes in any such room that may be constructed in the future. I will not go into details of the reasons, but will sketch briefly the isolated room as it should be.

The ideal isolated room should be in the centre of a special small building with nearly unbroken walls. If the building is far back from the street no special isolation by rubber is necessary; the room will be simply a part of the building. The room should be surrounded in every direction by experiment rooms, and should be separated from them by brick walls covered with tiles. These brick walls should be made practically air-tight. Various openings in these walls would allow experiments and observations from the outside. The blower should be in another building, and should have a large air-shaft and several sound-

killers. The most important improvement is to be made in furnishing the room. Persons entering our room through the heavy double doors for the first time are sometimes very nervous. Of course, as we seldom experiment on any but trained observers who are perfectly at home, this does not usually matter. Yet future work will doubtless cover cases where people enter a laboratory for the first time; moreover, it is a good principle to make every one at home. Therefore, the room should be furnished and lighted exactly like a comfortable room in the evening. All wires and apparatus should be concealed. The person entering it should suppose it to be only a reception room. He is to believe that he is merely on a visit; in a quite casual way he can be induced to pick up the key from a table and react to a concealed telephone or to a Geissler ornament. For the study of movements or of expressions a kinetoscopic camera can be concealed in some unsuspected place. Chairs, sofas, and rugs can be made to record movements. Proper control of the speed of the blower, or of an exhauster, and of the air supply, will render it possible to produce an atmosphere of any desired density, humidity, and temperature.

Among the modifications of apparatus made necessary by the complete separation of the subject from the experimental room, we must first notice the means of sending signals to the isolated room and at the same moment producing a spark-record on the drum. This is accomplished by means of the multiple key. The multiple key contains two levers revolving accurately around the same centre. When the key is at rest the lower lever at its rear end keeps two electric circuits closed. As the upper lever is made to descend by pressing the button, its two contacts in front strike the

corresponding two front contacts of the lower lever; this closes two electric circuits simultaneously. But at the moment this occurs the lower lever is forced to move, whereby the two rear circuits are broken. Thus two circuits are closed and two are broken at exactly the same moment. These combinations are necessary in various experiments. In the following illustration of reaction to a tone we shall need only one of the front, or make, and one of rear, or break, contacts. The other contacts on the key are for breaking or closing circuits

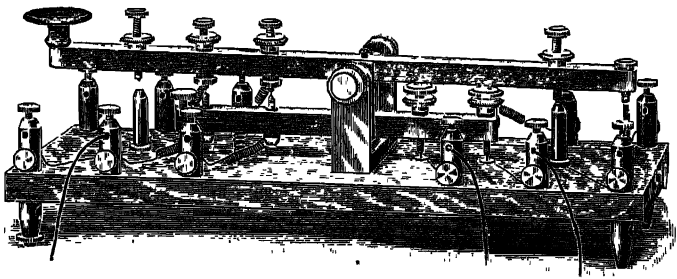


Fig. 34. MULTIPLE KEY.

just before or after the simultaneous contacts; we shall need only one of them.

To produce a tone in the isolated room a current is sent from a battery through a vibrating fork of the desired pitch, say 500 complete vibrations, then through a make-contact of the multiple key, and from here by wire to a telephone in the isolated room, and back again to the battery.[1] Nothing is heard till the key is pressed down.

[1] At Yale the connection is by a switchboard, similar to a telephone switchboard, which receives seven wires from each of eight of the rooms of the laboratory. The arrangement is shown in Fig. 208 of Scripture, "Thinking, Feeling, Doing," Meadville, 1895.

To register the moment of the production of the tone and of the response a current is sent through the spark coil, through one of the closed contacts of the multiple key, then to a reaction-key with closed contact (Figs. 23 or 27), and back to the battery. At the moment the front contact strikes, whereby the tone is produced, the rear contact breaks and makes a spark record. At an instant later the point on the front of the lower lever plunges into a mercury-cup, whereby the spark circuit is closed again, ready for the break of the key in the isolated room. At the moment the subject responds by pressing the reaction-key, a second spark record is made. The distance between the two sparks gives the time in thousandths of a second.

A typical experiment would be carried out in the following way : After shutting the doors, the observer takes his place in the easy-chair in the isolated room. Generally he will leave the electric light turned on, as it has been proven that a steady light is at least as favourable to mental action as darkness.[1] The reason probably is that in darkness we notice waves of light (so-called retinal light) rolling over space in front of us. Telephone connection enables the subject to communicate directly with the experimenter in the recording room.

The subject takes the reaction-key in his hand, and is ready to respond to the tone from the telephone which lies on the table. The click of a sounder warns him to pay attention. Shortly afterwards he hears the tone. As soon as he hears it, he moves his finger. This is all that concerns him directly. If he knows that for some reason he was inattentive, or if anything

[1] Bliss, *Investigations in Reaction-Time and Attention*, "Stud. Yale Psych. Lab.," 1892–93, i. 18.

else happened to disturb him, he telephones to the experimenter to cross off the record.

Meantime, what is happening in the experimental room? The experimenter starts the fork and drum going. He touches a key that produces the click of the sounder, the "warning," as it is called. About two seconds after this he presses the multiple key, whereby he sends the tone, and at exactly the same moment registers a spark on the drum. Shortly afterward a second spark is registered, coming from the reaction-key.

The experiments are repeated at intervals of about fifteen seconds. After the set of experiments is finished the paper is removed from the drum, run through varnish, and dried. The number of whole waves between each pair of sparks is counted and the fractional tenths are estimated. The results give the reaction-times, or "simple" reaction-times, as they are often called, in thousandths of a second. The numbers are placed on a record blank, and the average and the mean variation (p. 47) are calculated. The following is a characteristic record :—

151^{σ}	0.7^{σ}
147	3.3
153	2.7
141	9.3
142	8.3
155	4.7
158	7.7
155	4.7
151	0.7
150	0.3
150.3	4.24

What do these figures, $A = 150.3^{\sigma}$ and $MV = 4.24^{\sigma}$, mean?

In the first place, we know the apparatus to be exact to 1σ; therefore both the average and the mean variation are psychological quantities.

In the next place, the mean variation of 4.24σ means that in another set of experiments, under exactly the same circumstances, we can expect the subject to vary around his average of 150.3σ in a way indicated by his mean variation. This mean variation from ten records is of a size that indicates the writing of the results as 150σ and 4σ instead of 150.3σ and 4.24σ.

Finally, the question arises: Is the average to be stated to be 150σ when, for the sake of brevity, the mean variation is omitted? When we say that a line is 150 mm. long, we mean that its length is between 149.5 mm. and 150.5 mm. Likewise to say that the reaction-time is 150σ means that it lies between 149.5σ and 150.5σ. The mean variation shows that this is not true. Therefore, when we omit all mention of the mean variation, we cannot state the time to be 150σ. The time is really uncertain to about 5σ, but we are justified in stating it to be $15\,\Sigma$, where $\Sigma = 10\sigma = 0.01^{s}$. To state the result as 150σ without adding the mean variation is to produce an appearance of great accuracy which is really not present. When professedly scientific laboratory work is published with results reading to the thousandth of a second, while all indication of the mean variation is systematically suppressed, the procedure can hardly be said to be exactly correct.

The most important fact concerning the mean variation is its character as a psychological quantity. As we know the apparatus to be practically correct, the quantity MV must be attributed to the subject; and in just the same way as the reaction-time was treated as a psychological process, so we can consider this mean

variation to be a psychological affair. What is indicated by this mean variation, or index of irregularity, in regard to the mental processes of the subject? In physics we would say that such a mean variation indicated the average value of various sources of error, that as whole neutralised themselves when the average was taken. In psychology we say that it indicates the average value of various mental influences modifying our individual reaction-times to make them different from the average. These mental influences are, from one point of view, errors; from another they are residual phenomena awaiting investigation.

In explaining to visitors what the reaction-time is, I am generally met with the objection: "But different people differ." There is, in the first place, the vague notion that individual minds are unique and do not conform to any law; this is readily cleared up by a reference to a statistical investigation. Individuals do differ, but these differences occur according to definite laws, and the mean variation of the separate averages from the final average gives an index of this irregularity. In the second place, such a visitor does not comprehend that the purpose of the psychological laboratory is not ordinarily to collect statistics, but to determine fundamental laws. No two pieces of wire or anything else were ever exactly alike, but the physical laboratory is able to establish a law of relation between resistance, electro-motive force and intensity of current. No two cases of fever ever ran exactly the same course, yet the general laws governing various fever-diseases are quite capable of determination. No two minds were ever exactly alike, yet all follow the same general laws.

Proceeding with the investigation of the laws of reaction-time, we first ask: does the reaction-time differ for the different intensities of the sensation? Let us try

tones. The intensity of the tone is regulated by the intensity of the current sent through the telephone ; the amount of the current is governed by a resistance-box. The tone can be made of any loudness between unbearable intensity and silence. As no practicable method is at hand for measuring the intensity we must be content with such degrees as "very loud," "loud," "medium," &c. Experiments with tones of different intensities show no noticeable change in the average reaction-time.[1]

Does the time depend on the pitch of the tone? Experiments made with tones of 500, 250, and 100 complete vibrations gave for one observer 16Σ, 18Σ, and 24Σ respectively.[2] The pitch does change the reaction-time. Experiments by a somewhat different method give the following results for tones and a sharp click[3] :—

Tone	$C^{I}=33$	$c^{i}=264$	$c^{III}=1{,}056$	$c^{V}=2{,}112$	Noise.
1st Observer	17Σ	15Σ	14Σ	13Σ	11Σ
2nd „	16	14	13	12	12
3rd „	15	14	12	11	11

The reaction-time decreases as the pitch rises. The supposition that this decrease is due to a constant sensation-time of ten vibrations is contradicted by Martius's results, and the supposition that it is due to a constant sensation-time of three vibrations is contradicted by Slattery's. Tones of high pitch are more energetic, physically, than those of low pitch ; for the same amplitude of vibration the amount of work performed by a

[1] Martius, *Ueber den Einfluss der Intensität der Reize auf die Reactionszeit der Klänge*, "Phil. Stud.," 1891, vii. 469.

[2] Slattery, *On the Relation of the Reaction-Time to Variations in Intensity and Pitch of the Stimulus*, "Stud. Yale Psych. Lab.," 1893, i. 71.

[3] Martius, as before.

vibrating particle increases as the square of the number of vibrations. It might be supposed that the decrease in time for the high tones corresponded to an increase in the energy of the tone. This supposition is contradicted by the fact that, with a tone of constant pitch, the reaction-time does not decrease as the energy increases. The dependence of reaction-time on pitch thus stands as an unexplained fact.

Let us now turn to sight. When the stimulus is to be light, a Geissler tube is placed in the isolated room and is connected with the secondary poles of a spark-coil whose primary circuit is the same as that of the telephone used for the tone. When the key is depressed the tube is illuminated. The colour of the light can be varied by using tubes with different gases. The intensity can be changed by placing pieces of grey glass before the tube, or by allowing the tube to illuminate a white surface placed at various angles. In a series of experiments[3] in which the intensity of the light (though not from Geissler tubes) was widely varied, the results were as follows :—

Intensity .	1	7	23	123	315	1,000	*	**
Time in Σ	34	27	24	23	22	22	21	20

The intensity 1 was just visible ; the intensities above 1,000 were not numerically determined. The time is longer for weak lights than for strong ones.

For electrical stimulation an induction coil with a pair of electrodes can be substituted for the telephone used in reaction to tone. With electrical stimulation the reaction-time decreases steadily with increase of the intensity of

[3] Berger, *Ueber den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge*, "Philos. Stud.," 1886, iii. 63.

the stimulus.[1] The relation to the frequency of the shock has not been determined.

For the sense of touch the touch-key (Fig 33) is used. It requires a person in the room with the observer ; the multiple key is not used. The point of hard rubber is touched to the observer's skin. This breaks the electric circuit for an instant and makes a spark on the drum. To experiment with warmth and coolness, a metal ball is screwed on in place of the rubber tip and is heated or cooled as desired. Reaction to touch is quickest, coolness the next, and warmth the last. Fairly specimen figures would be : touch 11^{Σ}, coolness 12^{Σ}, warmth 13^{Σ}. Increase in the intensity of the temperature decreases the reaction-time. With a very hot or cold stimulus the reaction becomes very irregular owing to the sensation of pain which follows that of temperature.

In the foregoing experiments the stimulus has been varied and the results noted. Let us now vary the act used in responding.

The first point to consider is the particular act performed. With a stimulus applied to the arm the reaction time for the foot is 4^{Σ} to 5^{Σ} longer than that for the hand.[2] When it applied to the thigh, there is a similar but a much smaller difference of 1^{Σ}.

Still another variation is found in producing an extra muscular tension in the finger by having a weight pull against it. Pulleys were so arranged that a weight of 1 kilo. pulled upward on the reacting finger when the hand was put in position for reacting. The following is a characteristic record for a set of ten experiments on myself :—

[1] Berger, as before ; Slattery, as before.

[2] Dolley and Cattell, *Reaction-Times and the Velocity of the Nervous Impulse*, "Psychol. Rev," 1894, i. 159 ; also "Mem. Nat. Acad. Sci., U.S.," 1896, vii. 393.

	A.	MV.
Reaction to sound, without weight	172σ	30σ
" " with "	136	16
" light, without "	152	26
" " with "	128	11

Corresponding results were obtained from two other subjects.[1] There is a decided shortening of the time in each case, and a great increase in regularity. These changes were due (as was very evident to the subject himself) to the increased attention which the subject was forced, by the strain on the finger, to pay to the experiment. As the reaction-key was directly beneath the stimulus for sight, the increased attention made the sight reactions unusually short.

The length of the reaction-time depends upon the interval between the warning and the stimulus.[2] With intervals of $1\frac{1}{2}^s$, 3^s, and 6^s the reaction with sensory attention, *i.e.*, paid chiefly to the stimulus, steadily increases in length in a manner illustrated by the figures 26^Σ, 28^Σ, and 30^Σ respectively. For muscular attention, *i.e.*, paid chiefly to the reacting finger, similar but smaller differences exist, *e.g.*, 13^Σ, 13^Σ, and 15^Σ. When no warning at all is used the sensory reaction is slightly lengthened, *e.g.*, to 30^Σ, whereas the muscular reaction is greatly lengthened, *e.g.*, to 18^Σ. When the subject is taken quite unprepared for reaction, the time runs up to, say, 38^Σ.

When reactions are repeated rapidly in succession the problem becomes somewhat different from that of single reactions. In the latter the effort is made to keep the person's "attention" at the same level by means of a preliminary signal shortly before the experiment,

[1] Details will appear in "Stud. Yale Psych. Lab.," 1896, iv.

[2] Dwelshauvers, *Untersuchungen zur Mechanik der activen Aufmerksamkeit*, "Phil. Stud.," 1891, vi. 217.

and by ten seconds of rest between experiments. When, however, the reaction-stimulus occurs regularly at an interval of 2^s, each experiment serves as a preliminary signal for the following one. The subject, who knows about when the stimulus is coming, is to react to

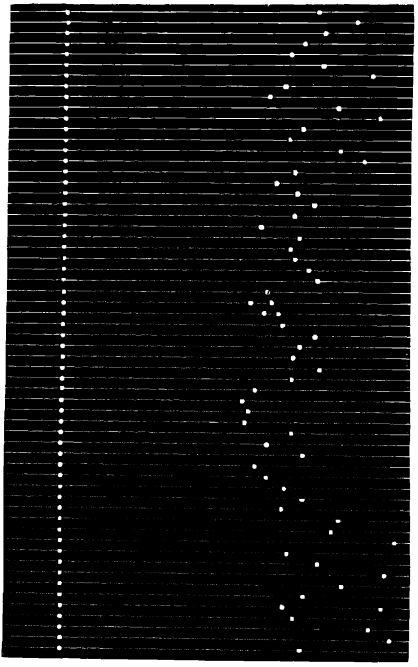


Fig. 35. A SERIES OF REACTIONS.

it as quickly as possible after he has received it. This involves an effort to shorten the time of sensation, and the time of action.

To perform the experiment a drum of the kind shown in Fig. 12, rotating regularly once in 2^s, is arranged to break an electric circuit at every revolution in such a

manner that a sound (or light, or electric shock) is produced at every revolution. The subject reacts with a break telegraph-key and produces a dot on the drum. In Fig. 35 the first dot on each line indicates the moment of the stimulus; the other indicates the reaction. At first the reactions are quite irregular in length; then they become quicker and more regular, showing practice; finally, they become longer and very irregular, showing fatigue.

Experiments made after this manner[1] show characteristic differences among people; for example, a physician and a student gave records similar to that of

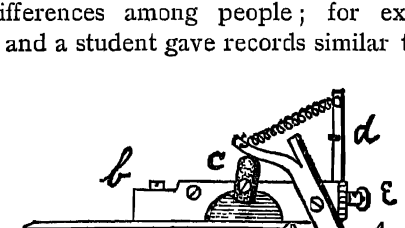


Fig. 36. PISTOL KEY.

Fig. 35, while a child of seven years gave a record with great irregularities, but with very little general lengthening of time due to fatigue.

Education in rapidity of reaction is obtained from various sports and games requiring quickness. Sprint racing is one of the most efficacious methods.[2] For investigating the changes in reaction-time due to

[1] Patrizi, *La graphique psychométrique de l'attention*, "Archives italiennes de biologie," 1894, xxii. 187.

[2] Scripture, *Reaction-Time and Time-Memory in Gymnastic Work*, "Ninth Ann. Meeting Am. Assoc. Phys. Ed.," 1894, 44.

practice in starting to run, I have devised a special pistol contact (Fig. 36).[1]

The blast from the pistol-barrel *a* moves the fan *f* so that contact is broken at *d* for an instant, the lever being drawn back by a spring. Only a few experiments have been made with the apparatus. These, however, have brought to notice these two facts :—1. The reaction-time is about one-third shorter for short-distance runners, who are trained to start quickly, than for long-distance runners. 2. The reaction-time for movements of the whole body is longer than for movements of a single member.

A remarkable illustration of the presence of reaction-time at a runner's start is to be seen in an instantaneous photograph reproduced in Fig. 37. The picture was evidently caught after the report of the pistol and before the start of the runners.

Let us, in conclusion, consider what a reaction-time means from a psychological and from a physiological standpoint.

Suppose we are sitting in the isolated room, waiting to react to the flash of a Geissler tube ; the flash appears, and we execute a movement of the finger. We feel that time was occupied by the appearance of the flash and by the action. We can divide the whole interval of time into two parts, the time for sensation and the time for action ; the relative proportions and the amount of time as recorded on the drum are unknown to us. Our apparatus states that the interval between the stimulus and the movement of the finger consisted of so many hundredths or thousandths of a second.

But how much of the reaction-time is psychological, how much physiological ? To the observer, as we have

[1] Scripture, as before ; also *Some New Apparatus*, "Stud. Yale Psych. Lab.," 1895, iii. 107.

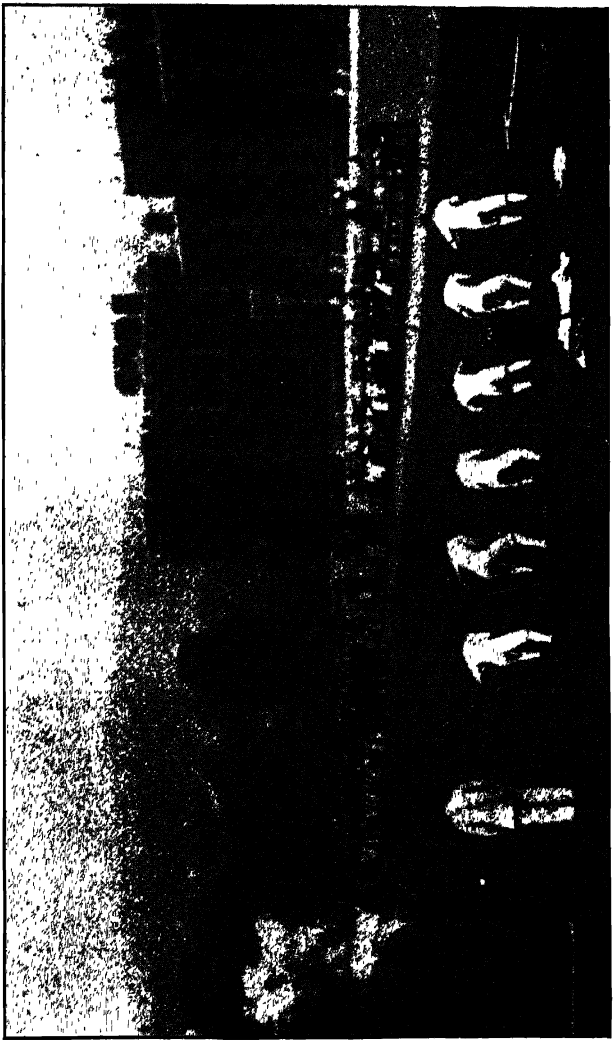


Fig. 37. THE RUNNER'S REACTION-TIME.

seen, the whole experiment consists in the appearance of the light and the will to act with the fingers. The various processes of recognition, choice, &c., have been practically eliminated by practice and warning signals. On the drum in the distant room he afterward sees a wavy line with two dots on it. By careful tests it can be proved that the Geissler tube flashed almost at the same instant as the spark made the dot. We can, therefore, assume that the production of the first dot and the production of the flash are two practically simultaneous occurrences in our visual field. The same holds good for the movement of the finger and the second dot. The number of waves between the two thus gives the time between the occurrence of the flash and the execution of the movement. Now, I do not say that we "see" the flash the moment the dot is made. For an object to be seen it must have a certain strength and clearness; we must be more or less fully conscious of it. This takes time. We do not really see the flash till it has been some time (of course, a very minute interval) in our visual field ready to be seen. This is the latent time of vision; it does not exceed 10σ. As the attention more concentrated, this time is lessened.

In regard to the finger that is made to move, we can readily prove that the more intense the effort of will, the more closely the finger movement as recorded coincides with the moment of the impulse. There is, however, always a minute interval of time lost after our impulse to act and the occurrence of the act as recorded. This we call the lag of the act; it does not exceed 4σ (p. 123).

We have so far not stepped out of the bounds of our mental experiences (our consciousness, as we call it), which really means that we have simply stated the facts as we know them; and we can take the recorded time as representing the interval between the entrance

of a flash into the visual field and the exit of an impulse to action out of it. The error of this assumption does not exceed 14σ.

On the physical side what happens? Our knowledge of physiology convinces us that, corresponding to seeing the flash, light entered our eyes and produced chemical changes in the retina, and that these were followed by some change transmitted along the optic nerve to the brain, along various paths in the brain, and finally down the spinal cord and the nerves of the arm to the muscles of the finger, which thereupon contracted.

We stand here before two sets of phenomena, each occurring in the same interval of time. In the first set, we experience the sensation and the volition directly: there is no doubt that they occupy practically all of the interval. About the second set, the chain of nervous phenomena, there is also no doubt. What is the relation between the two?

According to one theory, the theory formerly most prevalent, the nervous processes proceed to a certain point in the brain, where they give rise to mental processes. These mental processes continue for a while and then suddenly set the final nervous processes going. The reaction-time is thus the sum of the two series. According to another theory, the chain of nervous processes is unbroken; the reaction-time is entirely composed of the time required for the succession of these processes. The mental phenomena are only accidental attachments to members of this chain. According to still another theory, certain nerve elements are at the same time mental facts. In some the mental character is not so intense, in others more so. To ourselves the whole time is occupied by mental facts, some very dim, others more vivid. We must also believe, from our anatomy and physiology, that this

chain of facts is accompanied by a set of nerve processes. That we cannot scientifically grasp the identity of these nerve processes with mental facts, is due to the undeveloped condition of the philosophic thought of to-day.

Each of these three theories presents difficulties. It is hard to see how the first can be brought into relation with the universally accepted theories of physical phenomena. The second theory, that mental facts are nerve accidents, seems to be quite unintelligible in view of the laws of mental life which we discover. Our mental experiences are not purely capricious ; psychology is not a game of chance. Finally, that a nervous element can be mental or have a mental side to it, seems incomprehensible.

At any rate the question is not one of importance to the experimentalist. He must accept the mental facts just as they are given, and must measure and analyse them from the purely introspective standpoint. He cannot spend time on physiological speculations and anatomical metaphors. No profitable discussion of the relation of mind and brain can occur till the facts have been gathered on both sides. In the case of reaction-time nothing whatever is known of the intervals of time to be apportioned to the members of the series of nervous processes ; and we do not even know what many of these processes are.

CHAPTER IX.

TIME OF THOUGHT.

In previous chapters we considered the time of sensation and the time of action. Putting the two together we got the time of reaction.

By a proper arrangement of the methods of experiment various other mental processes in addition to sensations and impulses can be introduced into the reaction-time. The time for these additional processes may be called the time of thought.

As the intervals of time become longer, the experimenter generally prefers apparatus with which he can read the result at once instead of counting fork-waves. He resorts to the chronoscope method.

The usual chronoscopes were not invented for psychological purposes; they can be made to act properly only under so many precautions and inconveniences that the labour (although less than that of the older drum methods) is fully double that of the spark method I have described. These reasons have led to the construction of a chronoscope specially for psychological measurements.[1]

The pendulum chronoscope contains, in the first place, an accurately adjusted double-bob pendulum. This

[1] Scripture, *Some New Apparatus*, "Stud. Yale Psych. Lab.," 1895, iii. 98.

pendulum is held by a catch at the right-hand side (Fig. 38). In making an experiment this catch is pressed noiselessly and the pendulum starts its swing. It carries along a light pointer held in position by a delicate spring. At a definite moment it presses a delicate catch which releases the mechanism beneath the base. This mechanism is adjusted to do several things; one of them is to drop a shutter which covers an opening at the back of the chronoscope. The person experimented upon is seated at the back; owing to the curtain, he can see nothing but the covered opening. He finds before him a rubber button like that on a telegraph-key. He is to press this button as soon as he sees the shutter expose the opening. He does so, and another mechanism releases a horizontal bar running behind the scale. The pointer swings between this bar and the scale, and is consequently stopped when the bar snaps against the scale. The zero-point is passed at the moment the shutter starts to fall; the marks on the scale indicate the number of thousandths that elapse till the button is pressed. The instrument is built with the greatest accuracy. For reaction to light, coloured cards or pieces of transparent celluloid are inserted into a holder just behind the shutter.

The reactions to light are not disturbed by noises, as the pendulum makes no noise either at release or during its swing, and the shutter makes only a faint sound.

The shutter rests against a platinum point in such a way that its movement can be used to break an electric circuit; this can be used for producing lights, sounds, electric shocks, &c. A strong electro-magnet is placed beneath the base in such a way that it can take the place of the button; thus the pointer can be caught by the movement of a key in the hands of a distant person. An arrangement is also provided whereby the

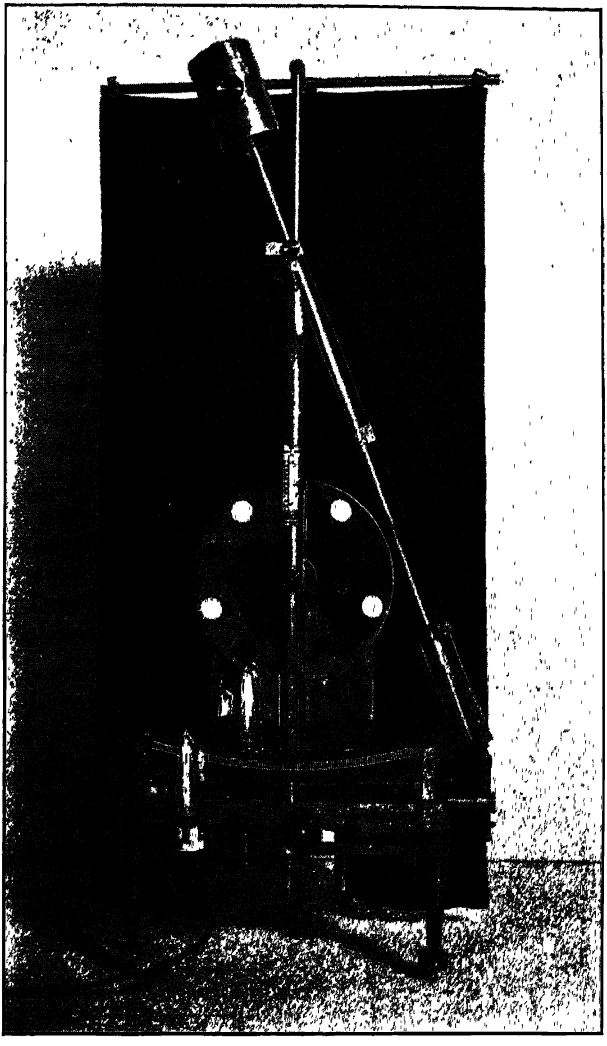


Fig. 38. PENDULUM CHRONOSCOPE.

pendulum itself is released electrically. Still further mechanisms are added for various purposes.[1]

Although the records are almost always read in thousandths, it is better to state them here only in hundredths, for the reason already given, namely, that the mental irregularity of the subject renders the thousandths uncertain (p. 142).

Suppose yourself comfortably seated behind the chronoscope with the finger resting on the button. The experimenter says, "As soon as the shutter flies down and exposes the red card behind it, press the button." Ten experiments are made in this way. The average and the mean variation are calculated and found to be, for example, $A = 25^{\sigma}$ and $MV = 3^{\sigma}$.

The quantity A has already been named the simple reaction-time; and it was found possible to divide it up into sensation-time and will-time.

You are seated again at the chronoscope. The experimenter says, "Here are two colours, red and blue. One of these colours will appear whenever the shutter falls. If it is red, press the button; if it is blue, keep still. Complete account of all mistakes will be kept."[2] He then shows one of the colours, generally blue for the first experiment. If you have just been making simple reactions, as is nearly always the case, you will press

[1] Fig. 39 shows the chronoscope with the curtain removed. The reaction-button is at the right. The arrangement is that used when the subject is in a distant room. The stimulus is sent by wires from the platinum contact. The reaction is effected by a battery and the magnet seen beneath the base. A record is supposed to have just been taken; the pendulum has swung to the further side, while the pointer is caught against the scale.

[2] The colours are inserted in the disc shown in Fig. 38. It is set in rotation and allowed to stop of its own accord. The subject's knowledge that the colours are chosen by chance and not by the experimenter makes the result much more regular.

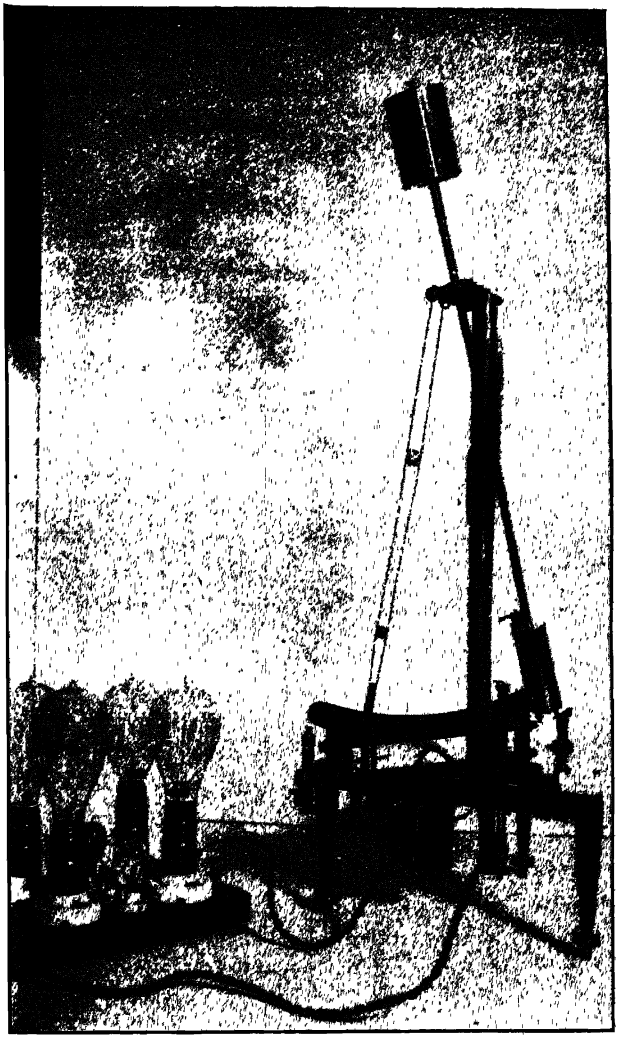


Fig. 39. PENDULUM CHRONOSCOPE, ARRANGED FOR DISTANT EXPERIMENTS.

the button. Try it again. You press the button when it is blue or you forget to press it when red. If you are a nervous person you will make several mistakes before you can get yourself to notice whether the colour is red or not before you react. After several experiments you settle down to a steady and reliable course of movement to red and no-movement to blue, say 38Σ. The time is 13Σ longer than for constant reaction to red when red is always present. The mean variation is also greater, say 6Σ. It is evident that the processes occupying this interval of time are more complicated; let us attempt to investigate these processes.

Instead of red and blue, three colours, red, blue, and yellow, are used with the understanding, "Reaction only to red." The time becomes, say 40Σ, with a mean variation of 8Σ. With more colours than three there is practically no change. By thus altering the number of objects among which the stimulus occurs, we find a difference in the effect.

Still another variation can be introduced. Instead of reacting to red and not to blue, you may react to red with the right hand and to blue with the left, both being on the button at the same time. The result is a trifle greater than before, say 41Σ, or an increase of 3Σ.

Again, let us use an electric key with five knobs arranged to stop the pointer of the chronoscope when any one is pressed. For red react with the thumb, and for blue with the index finger: result, say, 40Σ. Then for red and blue as before, and for yellow with the middle finger; result, 44Σ. Then as before, and for green with the ring finger; result, 47Σ. Again, as before, and for black with the little finger; result, 50Σ. Likewise with two five-finger keys and ten colours, the time steadily increases up to about 65Σ.

Two processes are concerned in the reaction-times

just measured; the one depends on the number of colours or objects that may appear, the other upon the number of different movements that may be called for. Without attributing any meaning to the names except as signs for what we have just found, we call these two processes, discrimination and choice. Thus, in our first experiment, the increase of 13^{Σ} over the simple reaction-time we regard as the amount due to discrimination and choice, when the discrimination lies between two objects and the choice between action and non-action. Likewise we get an increase of 15^{Σ} for three objects and action or non-action; 16^{Σ} for two objects and two arm-acts; 15^{Σ} for two objects and thumb or finger acts; 19^{Σ} for three objects and three acts; 22^{Σ} for four objects and four acts; 25^{Σ} for five objects and five acts; 40^{Σ} for ten objects and ten acts.

Methods have been proposed for determining how much of the total increase of time belongs to discrimination and how much to choice. For example, you are to react every time just as in simple reaction, but you are to wait till you feel sure you have discriminated. After what has been said on the unreliability of mere observation, this method is hardly to be regarded as trustworthy; for my part, I believe it better to leave the two together till they can be definitely separated by experimental methods.

You are now to perform another series of experiments of quite a different character.

You are seated comfortably with a special mouth-key at your lips, which is electrically connected with the magnet that stops the pointer.[1] Whenever you speak, you jar a light diaphragm, and this sends the current

[1] The key shown in Fig. 27 can be used for the same purpose by placing the knob under the chin; the subject is to begin each word by opening the mouth.

through the magnet. The experimenter says, "When the shutter falls, you will see a word or a picture.[1] Speak the word or name the picture." You do so a number of times, with a result, say, of 30^{Σ}. This is what is called a "word reaction"; it is composed of more or less complicated acts of sensation, discrimination, choice, and volition. For familiar words these processes are very rapid and take less time than our previous experiments would lead us, perhaps, to expect.

The directions are now changed to, "When the shutter falls you will see a word or a picture. Speak the first different word that occurs to you." The word "rain" appears; you say, for example, "snow": "good" appears; you say "bad": or the picture of a hat appears, and you say "James" (who owns a similar one), &c. You are at perfect liberty to say anything you please, provided it is the first thing that occurs to you. This is called "free" association.

Measurements of reaction-time and thought-time can be effectively demonstrated before a large class or audience by putting the platinum contact of the chronoscope in circuit with a lantern shutter held in place by a magnet. A slide containing a colour, a word, &c., is placed in the lantern, and the lens is covered by the shutter. As the pendulum passes the zero point the circuit is broken and the shutter falls, showing the colour or word on the screen. Some one in the audience reacts by means of a telegraph-key or a mouth-key connected by long flexible conductors to the magnet in the chronoscope. Every one in the audience sees the stimulus, and also sees the pointer stop after moving a certain distance.

Suppose that for the word reaction the results give 30^{Σ}, and for the free association 103^{Σ}. It is evident that the additional time, 73^{Σ}, is to be attributed to the

[1] These are placed in the disc shown in Fig 38.

difference between the processes involved in the two cases. It is generally stated that this time is that required for the first idea—*i.e.*, the presented word or picture—to "rouse up" the second one, *i.e.*, the word you speak. Or it is the time required for the second idea to "get into" or "rise into consciousness." These and the many other expressions are metaphors, and are absolutely meaningless as scientific terms. The plain facts are as we have stated them: Upon changing the instructions to the subject of experiment, an increase in the reaction-time is found. The second instructions differ from the first by calling for an "association of ideas," as it is commonly called. In just what the "association of ideas" consists we cannot say; we use it as a term representing the difference between the instructions. The increase in time corresponds to this change, and therefore we say that in your case, 73^{Σ}, was the time for association of ideas. It is to be kept in mind that the association-time is the difference between the reaction with association and the reaction without it.

It is soon noticed that associations that have been frequently repeated require less time. If your name happens to be John Smith you will probably not require more than 40^{Σ} for the association-time if you see the word, John, and associate Smith. If, however, you happen to make the association, John——the Baptist, the time may be 80^{Σ}, and, if you happen to call up some long-forgotten scene by associating John——George, the time may be, say, 110^{Σ}. This change in time as depending on the number and recentness of the previous repetitions may be used as a measure of their familiarity.

Up to this point you have been at liberty to think as you please; your associations were "free." They are now to be limited. In the first place, you are to give

the German name for every word and picture you see. Here you are limited to one particular answer. If you are rather weak in your German, you will find fairly long times, say on an average 60^{Σ}; if you are well up, the time will sink to something like 40^{Σ}; while great familiarity with the language brings the time down to 30^{Σ}. In all these cases you have frequently repeated the associations in learning the language, and they are more or less familiar. In the last case the familiarity becomes so great that you really do not associate at all, but simply perform word reactions; the groups of English letters mean to you not English but German words.

Various problems of this sort can be devised, *e.g.*, naming the following month, which takes half the time required to name the preceding month; naming the country to which a city belongs; performing arithmetical operations, &c.

Innumerable examples of such associations are to be found in school work and in practical life. Dull school exercises can be rendered wonderfully attractive to the children by making them perform the associations as quickly as possible; the most important point, however, is that the usual slow method of answering trains the child to be slow in thought at a time when he is most susceptible. Many schools are now arranging some of their lessons, *e.g.*, mental arithmetic, geography, spelling, &c., to serve as exercises in quick thinking.[1] How far such an education can go can be seen in the case of the telegraph operator, who translates visual English words into the Morse telegraphic language. The most rapid telegrapher in America has reached a speed of fifty-four words per minute. This is a

[1] Exercises with a revolving blackboard have been described in Aiken, "Methods of Mind Training," New York, 1896.

series of associations between visual impressions and arm movements; owing to the nature of the Morse alphabet, it approaches more an association between letters than between words. As the average number of letters in a word is five, this means 270 associations per minute, or about 22Σ for each. As the Morse alphabet requires several movements to form a single letter, this time is longer than that required for type-writing. Expert typo-telegraphers can with ease hear and write telegraph transmissions received at the above rate.

Instead of being strictly limited there may be a certain degree of freedom in the associations; you are, for example, required to name some part of a picture that is presented to you, or to tell some city of a given country, or some example of a class, or to add a subject to a verb, &c. The following are specimen averages and mean variations obtained on one individual.[1]

	A.	MV.
Picture—part	40Σ	10Σ
Name of object—property of object	44	16
Class—example	73	22
Adjective—noun	88	28
Verb—subject	77	37
Verb—object	65	24

Many of these limited associations are quite complicated. For example, suppose you are to associate the class to the object; the classes are "mammalia," "marsupials," &c. When "horse" is presented you think at once of "mammalia," but when "kangaroo" is used, you think first of "mammalia," but refuse to speak and think further till you get "marsupials." This latter case represents the actual process of what is artificially presented as a "logical judgment." Such cases form a considerable part of the limited associations.

[1] Cattell, *Psychometrische Untersuchungen*, "Phil. Stud.," 1888, iv. 242.

We have thus traced the processes of thought from the simplest forms of reaction to the most complicated that appear to have any regularity. The degree of regularity indicates the degree of control over the influences that affect the processes measured, and over the elements of which they are composed. Our scientific ability, however, stops at this point. It might, for example, be interesting to analyse and measure all the mental processes going on while a musician composes an overture, but it is just as impossible for psychology at present to do so as it is for physics to analyse the movements of a particle of air into the various notes of the orchestra when that overture is being played.

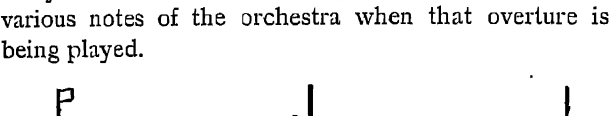


Fig. 40. THOUGHT AND ACTION APPARATUS.

The most promising lines for future investigations on the time of thought lie, I believe, in the study of particular cases with practical bearings. Such a concrete case is found in investigating the relations between reaction-time, thought-time, and action-time, which is provided for by the thought and action apparatus shown in Fig. 40.[1]

On a horizontal rod, one metre long, there are placed three metal blocks, A, B, C, adjustable at any distances apart. The block A carries a signal, and is arranged so that a movement of the signal breaks an electric circuit. The blocks B and C carry light bamboo rods held

[1] Scripture, *Some New Apparatus*, "Stud. Yale Psych. Lab.," 1895, iii. 106.

in small revolving clamps. On touching one of these rods it falls, and in doing so it makes electric contact for an instant. By connection with the spark coil records are obtained on the drum for each of the three movements.

Let it be required to determine the length of reaction-time in relation to the intended extent and velocity of movement. The finger is placed lightly against C, and upon a signal from A it is to be moved past B. The time between the sparks from A and C is the reaction-time; the time between those for C and B is the time of movement. By varying the distance BC and the time of movement the problem can be answered.

By omitting B the instrument serves as signal and reaction-key. By reacting to a movement of A in one direction and not in the other it is used for discrimination and choice. By using B and C alone it is an apparatus for repeating and extending the investigations[1] on the time and extent of movement.

Such an instrument affords the possibility of investigating to just what extent various athletic exercises tend to increase rapidity.

To illustrate the method of determining the educational value of sports and games I will give an example from some experiments on fencers. The visit of several expert swordsmen to Yale furnished the opportunity for experiments on their rapidity in some of the fundamental movements of fencing.[2] These experiments were made with cruder apparatus than that shown in Fig. 40, but the principles involved were the same.

[1] Fullerton and Cattell, "On the Perception of Small Differences," 103, Philadelphia, 1892.

[2] Scripture, *Tests of Mental Ability as exhibited in Fencing*, "Stud. Yale Psych. Lab.," 1894, ii. 122.

The first experiment included a determination of the simple reaction-time and of the time of muscular movement. The fencer stood ready to lunge, with the point of the foil resting to one side against a metal disc. A flexible conducting cord, fastened to the handle of the foil, hung in a loop from the back of the neck. A large metal disc was placed directly in front of the fencer at a distance of 75 cm.

By means of the spark method a record was made when a paper signal was moved, another when the fencer reacted by starting to lunge, and a third when his foil struck the large disc. The time between the first and second records gave the simple reaction-time; that between the second and third gave the time of movement through the given distance. About ten experiments were made on each person.

In a second experiment there was one piece of paper above, beside, and below the large disc. The fencer was to move his foil in the direction of whichever of these three papers moved. The acts of discrimination and choice were thus introduced into the reaction.

The persons experimented upon consisted of four expert amateur fencers, a professional trainer in fencing, a college professor, formerly practised in fencing, and another professor with no knowledge of fencing.

The experiments probably derive their chief value as calling attention to the experimental study of the psychological elements involved in games, sports, gymnastics, and all sorts of athletic work. Without experimenting on large numbers of fencers and others, I would not attempt to make any quantitative comparisons between the two. The following qualitative conclusions seem, however, to be fully justified. The average fencer is not quicker in simple reaction (where a few mental elements are involved) than a trained scientist, and

neither class shows an excessive rapidity. When once the mind is made up to execute a movement, fencers are far quicker in the actual execution. In rough figures, it takes them only half as long as the average individual. The quickest one in these experiments was the fencing master. As the mental process becomes more complicated, the time required by the average fencer is greater than that required by a trained scientist, although the latter may be unfamiliar with fencing. The slowest one in these experiments was the fencing master. The general conclusion seems to be that fencing does not develop mental quickness more than scientific pursuits, but it does develop to a high degree the rapidity of executing movements. It would be important to determine if this holds good of the other sports and exercises, or if some of them are especially adapted to develop mental quickness.

CHAPTER X.

TIME ESTIMATES.

HAVING measured our thoughts in terms of time, let us measure time by our thoughts and see what result we get. The general standard of time has been established in Chap. V.; we will now investigate auditory, visual, tactual, and other times, and see how they compare with standard time.

To show how this is done I will first describe my own method of performing the experiments, as being the simplest and most easily explained. Thereafter I will state the results reached by various investigators.

An electric tuning-fork is kept vibrating by means of a battery. The current passes also through the primary coil of an inductorium. Wires lead from the secondary coil to a distant room. In this room the current passes through a telephone. The interruptions of the current in the primary circuit produce momentary currents in the secondary one; thus the telephone produces a tone of the same pitch as the fork. The intensity of the tone depends on the distance between the two coils and on the strength of the battery.

An accurate clockwork causes a pointer to revolve at the required speed. At a certain moment it makes an electric contact whereby a modified telegraph relay is made to close the tone-circuit. At another point the

relay is made to break the tone-circuit; thus a tone of definite length is produced. Immediately thereafter the tone-circuit is again closed and the tone continues. Finally, when the second tone seems as long as the first the subject of experiment presses a key, whereby a spark record is made on a drum attached to the clock work.[1]

When the time-intervals are to occur in other senses the appropriate changes are made in the apparatus. For skin stimulation, the telephone is replaced by electrodes. For sight, the induction coil with telephone is

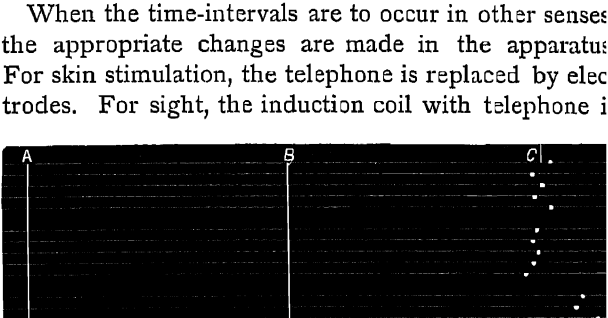


Fig. 41. RECORD OF TIME ESTIMATES.

replaced by a spark coil with a Geissler or a Pulu tube whereby a light is produced. In the following treatment the illustrations will be drawn from the sense of hearing; other senses show, as far as investigated, perfectly analogous results.

The record of a series of such experiments is shown in Fig. 41. The tone begins at the moment A, and is interrupted for an instant at the moment B. The second tone is marked off by the dots at C, whereby

[1] The other methods of investigating the time sense, namely, the of minimum variation and of right and wrong answers, require the addition of another contact in place of the key and spark-record.

BC is supposed by the subject to be equal in time to AB. The figure is a copy of records for four different observers, evidently of different mental constitution. The first observer is correct in the average and is fairly regular; the second is extremely regular but shortens the time; the third is behind time but fairly regular; and the last is very irregular but correct on the average.

In these experiments we have two tones judged by the ear to have lasted equal times. When compared with standard time, however, these tones may or may not have agreed in length of duration. By assuming the first tone-time to be the standard with which the second was compared, we get the error of comparison in terms of the first interval of time. In this sense we speak of the first interval as the standard interval.

The results of the experiments (in which I myself was the subject) are shown in the following table :—

Interval.	Estimate	D.	D. %	MV.	MV. %	N.
5.3^{Σ}	5.4^{Σ}	$+ 0.1^{\Sigma}$	+ 2 %	0.5^{Σ}	11 %	31
13.5	8.7	— 4.8	— 4 %	1.2	14 %	30
92.	80.	— 12.	— 13 %	10.	17 %	35
220	154.	— 66.	— 30 %	27.	17 %	11

The column Interval gives the length of the constant interval of time; Estimate gives the average length for the second interval as marked off by the subject; D. is the difference between the two, and D. % this difference expressed as a fraction of the standard. MV. is the mean variation in the results, and MV. % this variation expressed as a fraction of the standard. There appears in the subject a slight increase in the relative uncertainty

of judging time as the interval increases. The column N. gives the number of experiments made on each interval.

It will be noticed that with the short interval of 5.3$^{\Sigma}$ the error was an extremely small over-estimation; while as the interval was increased the error became an under-estimation, which steadily increased in size. This corresponds in one respect to the work of previous investigators, who have found that larger intervals are under-estimated.

From this point the investigation should proceed to the study of intervals of half, quarter, double, triple, &c., and the various kinds of sensation should be considered. I am not aware that this has been done.

Experiments of the kind described above were made by Gilbert,[1] with simpler apparatus, on one hundred children of each age from 6 to 17, of the city of New Haven, Conn. The standard was a tone of 100 v. d., lasting two seconds. Out of 1,192 children only thirty-eight made the second interval longer; all the rest shortened it. A few of the younger children made the second sound not quite half as long. The error steadily decreased from 29 % at 6 years to 20 % at 17 years. It is evident that the second interval must have seemed very long indeed to the younger children.

Most of the investigation of time-estimation has been done with so-called "empty" intervals. The intervals are marked off by sharp clicks. Changes are made in the apparatus previously described, whereby the relay and telephone are replaced by a telegraph-sounder; the tuning-fork becomes unnecessary. In this manner short, sharp clicks can be made to occur at any time desired; the loudness of the click depends on the

[1] Gilbert, *Mental and Physical Development of School Children*, "Stud. Yale Psych. Lab.," 1894, ii. 52, 86.

strength of the battery. The intervals between the clicks are not really empty; they are filled with our thoughts. Still, our thoughts generally proceed with a fair amount of evenness.

The error made in estimating time intervals depends on the mental condition. The error is decreased by strict attention, by practice, and by a stimulant like tea; it is increased by fatigue and by alcohol.[1] On the other hand, the extreme lengthening of time resulting from the use of hashish does not seem to involve incorrectness in estimating intervals.[2]

Time seems longer or shorter according to the way in which it is filled. The following results of investigations on this point are all from Meumann.[3]

When two intervals are compared, of which one contains extra clicks and the other is empty, there is a difference in estimating them. The characteristic relations of the two are shown in Fig. 42, where the dots indicate clicks.

Fig. 42.

The interrupted time AB is compared with the empty time BC. Those familiar with optical illusions will expect that the interrupted time seems longer than the empty time. This is true, however, only of small intervals. As the interval increases in length the overestimation of AB decreases till at an indifference-point

[1] Eijner and Kraepelin, as cited in Wundt, "Physiol. Psych.," 4. Aufl., ii. 414, Leipzig, 1893.

[2] Scripture, *Consciousness under the Influence of Cannabis Indica*, "Science," 1893, xxii. 233.

[3] Meumann, *Beiträge zur Psychologie des Zeitbewusstseins*, "Phil. Stud.," 1896, xii. 127

(whose value depends on the number of intermediat clicks and on the observer) AB=BC; with large intervals AB is under-estimated. Analogous result are found for time intervals of sight and touch.

When an interval filled with a tone is compared witl an empty one, the result depends on which comes first When the tone-interval is first, the empty interval i under-estimated. When it is second, the tone-interva is over- or under-estimated according to the length o the interval.

When time is filled with mental work, such as i reading off letters from a rotating drum, the estimate i influenced. The subject is told to mark off an empt time after the last letter which seems equal to the tim during which he was occupied in reading. With on observer the empty time registered on an averag 4.8^s for a reading-time of 5.0^s; with another it wa 6.4^s for 8.0^s; with another 4.0^s for 4.8^s. In anothe case the observer was required to count the number o lines passing across an opening; the empty time wa made 4.2^s for a filled time of 5.0^s.

Our estimates of empty intervals are influenced b the sounds used to mark off these intervals. A con venient method of making experiments on th's problen is to produce a series of clicks at regular interval under one condition and then a series under the nev condition.

When a series of 50 weak clicks at intervals of 25 is compared with a similar series of 50 loud clicks a the same interval, the observer, knowing nothing o the actual rates, judges the louder series to have bee more rapid.[1]

Likewise, as the clicks of a series are gradually

[1] Meumann, *Beiträge zur Psychologie des Zeitsinns*, "Phil. Stud.," 1893, ix. 209 (274, &c.).

strengthened or weakened, the interval between seems shortened or lengthened.

The same relation is true for sight. If a series of sparks be seen through the glass wall of a sound-tight box, the sparks seem to come more rapidly if stronger.

If a stronger click be inserted in the middle between weaker ones, the first half of the interval appears to be shorter than the second half. Small intervals in which series of momentary events occur appear longer than empty intervals ; as the size of the interval is increased this difference steadily decreases and finally becomes just the opposite.

It is evident that the length of a time-interval depends on the quality of the limiting sensation. This leads us to the question of the relation between emphasis and time in rhythm. Meumann's experiments prove that emphasis, location, and length of interval are related and inter-dependent factors of rhythm.[1] For example, an emphasised click at the beginning of an interval makes it seem actually longer than the following intervals with unemphasised clicks.

But it is evident that we have arrived at the form of time-estimate known as rhythm ; this we will reserve for the following chapter.

[1] Meumann, *Beiträge zur Psychologie des Zeitsinns*, "Phil. Stud.," 1893, ix. 306.

CHAPTER XI.

RHYTHM.

THE purpose of rhythm is to aid in clearness of the feeling for time intervals. This aid is derived from the regular construction of the forms of rhythm, whereby we grasp more complicated periods as if they were simpler ones.

Pieces of music are built up of bars, each one with its accents; these again are combined into cadences; greater units are found in the stanzas.

In poetry the rhythmic form is given by length, by emphasis, or by both. Ancient poetry made use of syllables of different lengths, presumably combined with emphasis. Modern poetry nominally makes use only of emphasis.

The problem of emphasis-rhythm has been investigated by Bolton.[1] An apparatus was so constructed that a sequence of sounds was produced which did not vary either in pitch, intensity, or quality. The rate at which the sounds were made to succeed one another could be varied from one in two seconds to ten in one second. When a person listened to the series and gave his attention closely to it, the series seemed to break up into groups of sounds; it did not appear uniformly continuous. These groups contained a larger or smaller number of sounds

[1] Bolton, *Rhythm*, "Amer. Jour. Psych.," 1893, vi. 214.

according as the rate was fast or slow. If the rate was slow, groups of two sounds seemed the more natural. At a faster rate grouping by threes or fours was more easy and pleasing, and with the fastest rates the sounds seemed to group either by sixes or eights, and sometimes the sequence seemed to rise and fall in intensity at regular intervals of one second or more. The grouping was not distinct. Whatever the rate, the sounds might be made to group by suggesting to the subject a pendulum or some other rhythmical instrument. Groupings might be suggested by counting 2's, 3's, 4's, 6's, or 8's, accenting the first sound. It was difficult and even impossible with most persons to group by 5's or 7's. When the subject gave his attention to breathing, the sounds were grouped accordingly. The beginnings of the groups corresponded with the acts of inspiration. Some persons who were used as subjects knew that they were predisposed to a particular rhythmical form. One always delighted in fours ; four had been his number in school. Another had from childhood been accustomed to group the ticks of the clock by fours, so also the puffs of a locomotive when it was starting or pulling up a grade. Another had delighted in grouping the figures of the wall-paper or other serial objects into fours ; he always counted by fours, and seemed to have a feeling for this number that other numbers did not give him. In general, whatever there was to call the subject's attention to any number, suggested a rhythmical grouping for the sounds in the sequence.

This grouping was accompanied by some kind of muscular movement. Frequently it was tapping with the foot or the fingers ; sometimes it was beating time with the hand or the thumb. Some subjects nodded the head, others counted inaudibly, and still others

felt indefinitely localised muscular contractions in the larnyx, diaphragm, viscera, scalp, eyelids, &c. Muscular twitchings were to be seen in the muscles of the face and limbs at times when the subject declared he felt nothing of the kind.

The grouping was accomplished by placing a stress or accent upon the first sound in a group. In groups of three the first and second sounds were accented, the first more strongly than the second. In groups of four the first and third were accented, the first again being the stronger. Groups of four seemed at times to break into two groups of two sounds each, groups of six into two groups of three, and groups of eight into two groups of four. In groups of six the accents came always upon the first and fourth, and in groups of eight upon the first and fifth. The accents were apparently at the basis of the splitting up of the longer groups, and, when they did so break up, the subject felt a tendency to swing forward or backward or from side to side. This invariably suggested the pendulum. Many persons pictured or visualised some moving object which seemed to swing or revolve as the sounds were grouped.

When the various rates at which the different groupings were felt to be most pleasing and natural were compared and the average times for each taken, it was found that the time limit of each group was nearly the same—a little more than a second. The explanation of this was based upon the rhythmical character of the attention. Attention is periodic, and, when it is concentrated upon a continuous series, becomes quite regular in its periods. An object that does not change cannot be attended to for more than a few seconds. The attention will pass involuntarily from the object to some one of its parts or to one of its associates.

Our time estimates furnish the basis for rhythmic movements. By rhythmic movements we understand movements repeated at apparently equal intervals.

As the first problem let us ask the question: What are the most natural time-intervals chosen for rhythmic movements, and what is the course of the movement during the interval?

The subject of the experiment holds a light rod between his thumb and finger and moves it up and down continuously through a short distance at any rate he desires. This rod is provided with a light point, adjusted to write on a steadily revolving smoked drum. The whole course of the movement is thus recorded. The experimental arrangements are similar to those of Fig. 12, omitting the rotating disc.

In experiments I have made on six people the intervals chosen were, on an average, 92, 94, 152, 156, 160, and 180$^{\Sigma}$. These averages were derived from ten successive movements. Their mean variations give, therefore, indices for the irregularity with which the interval was maintained; they were 4, 11, 7, 8, 9, 6, 8, and 4$^{\Sigma}$ respectively, showing in most of the subjects a remarkable fixity of the interval.

Experiments of a somewhat similar kind have been made by Stevens.[1] The subject had to make movements in time with the beats of a metronome, and to continue them at the same rate after the metronome stopped. The movements were recorded on a smoked drum, and the average interval of the movements without the metronome was compared with that for the movements with the metronome. The two sets agreed only when a particular interval was used; each subject possessed a peculiar interval, which ranged from 53$^{\Sigma}$ to 87$^{\Sigma}$.

[1] Stevens, *On the Time Sense*, "Mind," 1886, xi 393.

It is a fact of common experience that each person or animal has a natural rhythm for his particular activities. This natural rhythm is the one in which he can perform the greatest number of movements with the least fatigue. If we walk faster than a certain gait we readily become tired, and strolling, which is walking at a very slow gait, is an extremely fatiguing method of progress. A bicyclist who rides faster or slower than his usual rate goes a much smaller total distance than otherwise. Similar facts are known to all intelligent drivers of horses. Just what are the usual rhythms for the various voluntary activities remains a subject for investigation.

The particular rhythm chosen is, however, in practical life frequently of less importance than its regularity. An orchestra leader, for example, may beat a trifle faster or slower without much damage, but at any rate he must beat regularly.[1] Every musician likewise must learn to be regular in his intervals, though he may to a certain extent choose their average length.

The rhythmic actions just considered may be said to be "free"; they become "forced" when the actions are performed in time to some regularly repeated signal.

To perform experiments on forced rhythm electric contacts are so adjusted to clockwork that a sound or a flash is repeated regularly at the desired intervals. The subject keeps time with some kind of a key. By use of a spark coil dots can be obtained on a time-line at the moment the responding movement is executed.[2]

[1] Methods of performing experiments are to be found in Scripture, "Thinking, Feeling, Doing," ch. xix., Meadville, 1895.

[2] A better method—lately perfected—of performing experiments on rhythmic action is shown in the frontispiece. As the recording drum revolves it strikes an electric contact which produces a click of the sounder; the subject keeps time with a key. The drum

To illustrate the characteristic differences between people I give in Fig. 43 a series of records made on students. Clicks of a telegraph sounder occurred regularly at intervals of half a second. The subject kept time by tapping a telegraph-key, which made a spark record on the drum. It is worth noting that the second was a musician.

The mental processes involved in rhythmic action of this sort are derived from those involved in time estimate and in reaction-time. The subject does not simply react to the sound; he anticipates the moment of its recurrence, and reacts in some relation to this antici-

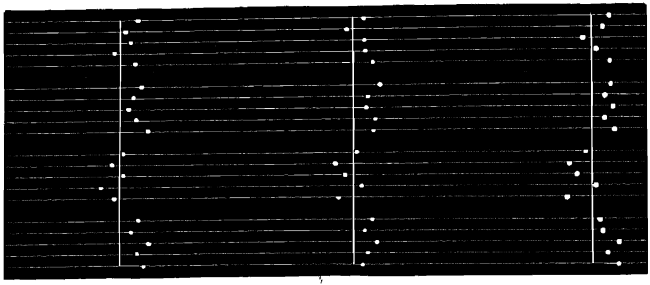


Fig. 43. RECORDS OF RHYTHMIC ACTION.

pated moment. The particular relation depends upon the individual. Some persons almost react to the sound itself; others react by anticipation so that the actual movement occurs simultaneously with the sound; still others unconsciously react so far ahead that the movements actually precede the sound. By averaging all

with the recording-fork is shown at the right, the sounder in front, and the subject with key at the left. The spark coil and the motor for the drum are seen at the back. The electric currents for the key, sounder, and fork are drawn from the lamp batteries at the top. The subject and the sounder may be transferred to another room.

the errors + and — made by the subject we get his constant error. A person with an error of o is one that on the average brings his finger down in exact time to the sound. A + error indicates that on the average he is behind time ; a — error that he is ahead of time.

In forced rhythmic actions, as well as in free, the mean variation, or index of irregularity, is even more characteristic of the individual than his average time. In finding the average time the subject's irregularity disappears. Two subjects might have o as their constant errors, yet one of them might be extremely irregular and the other one almost perfectly regular.

It is remarkable that the subject of rhythmic action has received so little attention in connection with investigation of time estimates. In fact, I know of only one set of experiments,[1] and these, published in a medical periodical and accessible only at second hand,[2] appear to have been of very little extent. Two persons took part in the experiment. The one struck a key as regularly as possible, the other kept time with him. According to the records the irregularity of the first person did not exceed 3σ and a disagreement of less than 4σ between the two persons was not perceptible.

These experiments naturally involve much greater complications than those where the signal is given by clockwork, because the person marking time has a more or less irregular sound to agree with. The conditions are, however, closely similar to those in actual life, *e.g.*, in marching in time to a piano, drum, &c., played by some person.

To investigate the concrete case of keeping time to a drum beat I make the following arrangement: A metal

[1] Martius, "Zt. f. klin. Medizin," xv. 536.

[2] Schumann, *Ueber die Schätzung kleiner Zeitgrössen*, "Zt. f. Psych. u. Phys. d. Sinn.," 1892, iv. 45.

plate is laid on the top of the drum and metal knobs are substituted for the wooden ones on the drum sticks. The sticks and plate are placed in the primary circuit of a spark coil so that records may be made on the smoked drum. The person marking time to the drum wears an electric contact attached to the heel [1] whereby his movements are likewise recorded. The marching records of a trained soldier are as accurate as those of a musician. The drummer varies in his intervals much the same as an orchestra leader.

[1] Scripture, *Some New Apparatus*, "Stud. Yale Psych Lab.," 1895, iii. 108.

CHAPTER XII.

TIME INFLUENCE.

In the chapters on sensation and action we noticed the changes in our experiences during short intervals of time; we have now to consider the effect of long intervals. Suppose that we are looking at one of the illustrations in this book. At a certain moment t_0 we close the book. At a certain time afterwards t_1 we can, by appropriate means, recall the illustration to mind. How does the recalled illustration differ from the original? What is the law according to which this difference depends on the elapsed interval of time $t_i - t_0$ where $i = 0, 1, 2,$ &c.?

To illustrate the phenomenon of memory let us perform the following experiment. Sheets of paper are numbered in succession from 0 to 10. No. 0 has two dots near the middle at 50.0 mm. apart; the others have one dot each corresponding to one of the dots in No. 0. The pad is laid on a table. The eyes are run once over the distance between the dots on No. 0, and are then closed, while a clock or a metronome ticks 5^s or an assistant observes the time. Meanwhile No. 0 has been removed and the hand with a pencil is held ready for use. The eyes, upon being opened, move from the single dot of No. 1 to where the other dot ought to be in order to correspond to No. 0; this point is marked by the pencil.

No. 1 is laid aside, and after a few moments' rest No. 0 is again placed before the eyes and the experiment is repeated. This is done ten times. The result is a series of distances reproduced by eye-memory at an interval of 5". A set of such experiments gave, for example, the following results :—

	55.1		1.8
	57.9		1.0
	60.0		3.1
	55.1		1 8
	55 6		1 3
	57.9		1.0
	56.7		0.2
	57 1		0 2
	56 2		0 7
	57.5		0.6
Average memory image	56.9	Mean variation or uncertainty	1.2
Original...	50.0		
Constant error	6.9		

The results, when stated as rounded-off percentages, are $A = \frac{56.0}{50.0} = 114\ \%$, $C = \frac{6.9}{50.0} = 14\%$, $M = \frac{1.2}{56.9} = 2\ \%$.

The average result gives the condition of the sensation at the end of 5". The difference between this average and the original distance shows the amount of inaccuracy due to the lapse of the 5". In this case it is 6.9 mm. The mean variation of 1.2 mm. shows the amount of uncertainty in the sensation at that time. Both of these latter figures are errors of memory. The first one we may call the inaccuracy of the memory, C, and the second one the uncertainty of the memory, U, at the moment $t = 5"$. The same experiments are repeated with intervals of 10", 15", 20", &c. The problem of memory is to investigate the dependence of these two errors upon the interval of time, that is, we must determine the functions $C = f(t)$ and $U = F(t)$. When

the forms of these functions have been determined the law of memory will be found.

This simple and accurate method of investigating memory—I had almost said: the only scientific method—has not been much used; I can therefore give comparatively few illustrations.[1]

Binet and Henri[2] have used this method for testing the memory in school children. With lines of 16, 40, and 68 mm. the inaccuracy of the reproduced line consisted in an increase in length; with the lines of 4 mm. and 1.5 mm. it was a decrease. The account gives no further indication of the laws of memory.

In experiments performed by Loewy, the subject was touched on the skin of the arm;[3] after an interval of t seconds he indicated with a pencil the point touched. The error made gives an indication of the influence of the elapsed interval. Results were obtained as follows: for $t=0$, $C=1.0$ mm.; for $t=20$, $C=1.3$ mm.; for $t=120$, $C=2.2$ mm.

For the memory of arm-movements experiments have been made at the Yale laboratory with a specially constructed apparatus. It consists essentially of a horizontal wooden scale having a smooth glass tube for its upper edge. At the zero point in the middle there is a fixed metal plate. On either side there is a movable slide;[4] only one side is used in the present experiments. The subject, with eyes closed, places his forefinger against

[1] It seems to have been first used by Hegelmayer ("Vierordt's Archiv," xi. 844) whose few experiments showed that U increased with increase of t.

[2] Binet et Henri, *Recherches sur le développement de la mémoire visuelle des enfants*, "Rev. Phil.," 1894, xxxvii. 348.

[3] Loewy, *Experimentelle Untersuchungen uber das Gedächtniss*, "Zt. f. Psych. u Phys. d. Sinn.," 1895, viii. 271.

[4] These slides contain adjustments for compensating the width of the finger.

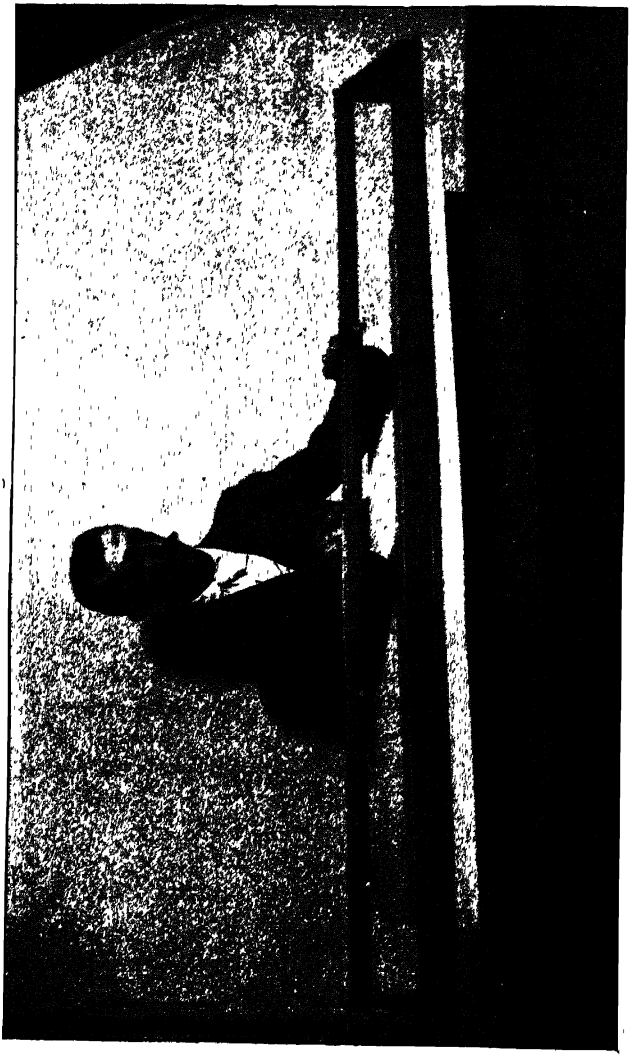


Fig. 44. INVESTIGATING THE ARM MEMORY.

the zero plate. He then moves it outward till it strikes the slide which has been placed at some given distance. The finger is then moved back to the zero plate and kept there for 10". Meanwhile the slide has been moved out of the way. At the end of the 10" the finger is moved outward again to what the subject considers to be the same distance as before. This is repeated with an interval of 20".

The results given in the following table [1] are averages of fifty experiments each on the distances 100 mm., 300 mm., and 500 mm.

Subject.	C.			U.		
	0"	10"	20"	0"	10"	20"
A	+ 2.3	+ 2.8	— 0.1	0 9	1 7	1.3
B	+ 2.8	— 2 1	— 0.3	1.0	1.3	1.4
C	— 1.0	— 3.1	— 3.7	0.9	1.1	1.2
D	+ 0.2	— 2 7	— 4.0	1.3	1.4	1.9

It will be seen that the constant error slowly changes with the increasing interval, the particular course of the change depending upon the individual; the uncertainty, however, steadily increases. Whether our memory images become erroneous with the lapse of time or not, they at any rate become continually more indefinite.

Beaunis [2] has made a series of experiments in the following manner. With eyes closed, he drew a line

[1] The detailed results will be published in "Stud. Yale Psych. Lab.," 1896, iv.

[2] Beaunis, *Recherches sur la mémoire des sensations musculaires*, "Rev. Phil.," 1888, xxv. 568; also in Beaunis, "Nouveaux éléments de physiologie humaine," 3rd ed., ii. 793; Paris, 1888.

on a sheet of paper, and then after an interval of 5^s he drew another one beside it, which was intended to be its equal. The experiments were made with intervals of 5^s, 10^s, 15^s, ..., 50^s. The investigation covered lines of various lengths. In another set of experiments, distances were marked off by dots instead of lines. In a third set angles formed by two straight lines were used.

Beaunis draws several conclusions, partly from the results and partly from introspective observation. In the first place, the muscular sensations (by which he means the sensations of motion, pressure, &c., arising from the movement of the arm) do not disappear gradually, but vanish suddenly after an interval. When the memory of the line or the angle had been lost so that the observer could not tell what it was, *e.g.*, whether an acute or an obtuse angle had been traced, the hand could in certain cases, and during a certain period, reproduce the original exactly. At a somewhat later moment, even this would be impossible. There are, therefore, says Beaunis, three phases in the disappearance of a muscular sensation: (1) of conscious memory; (2) of unconscious memory; (3) of total forgetfulness.

It was noticed by Weber and Fechner[1] that the memory for arm movements is transferred from one side to the other and that this cross memory is symmetrical and not identical. After we have learned to write with the right hand our left hand is also found to be able to write passably well outwards, *i.e.*, backwards, but not so well in the regular direction.

[1] Fechner, *Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite, die der anderen zugleich mitgeubt werden*, "Ber. d. k. sächs. Ges. d. Wiss., math.-phys. Cl.," 1858, x. 70.

To investigate this cross memory lines were drawn alternately with the right and left hands in the following directions : R⟶, L⟵, R⟶, L⟶. The difference was then found between each line for the left hand, and the preceding one for the right hand. With opposite movements the line with the left hand was made on the average 8 mm. too short, while with identical movements it was made 12 mm. too short. This was on a line averaging 50 mm. The average uncertainty was the same in both cases, namely, 5 mm. These results were obtained for a single interval, and for a single person, namely, myself ; it is desirable to know if these peculiarities would be found with other intervals and persons.

Wolfe [1] has made a series of experiments on memory for tones according to the statistical method. A tone was produced on a reed instrument that maintained it for one second ; after an interval of t seconds either the same tone or one differing from it by a definite amount was likewise produced for one second. The observer was required to say whether the second tone was the same as the first or not. The number of times that he answered correctly depended on the length of the interval t. For example when $t = 1^s$ an observer would answer correctly 93 times out of 100 ; when $t = 2^s$, 92 times ; when $t = 3^s$, 89 times, &c. The influence of the elapsed time thus shows itself in a decreased percentage of correct answers.

By mere chance the answer would be correct in fifty per cent. of the experiments ; any increase above this amount is due to some effect from the former tone lasting over the interval of time. This we ordinarily say is the effect of the person's memory of the tone ;

[1] Wolfe, *Untersuchungen über das Tongedächtniss*, "Phil. Stud.," 1886, iii. 534.

more properly we should say this is the memory of the tone. Experiments on nine subjects with intervals from 1" to 60" give results as shown in Fig. 45.

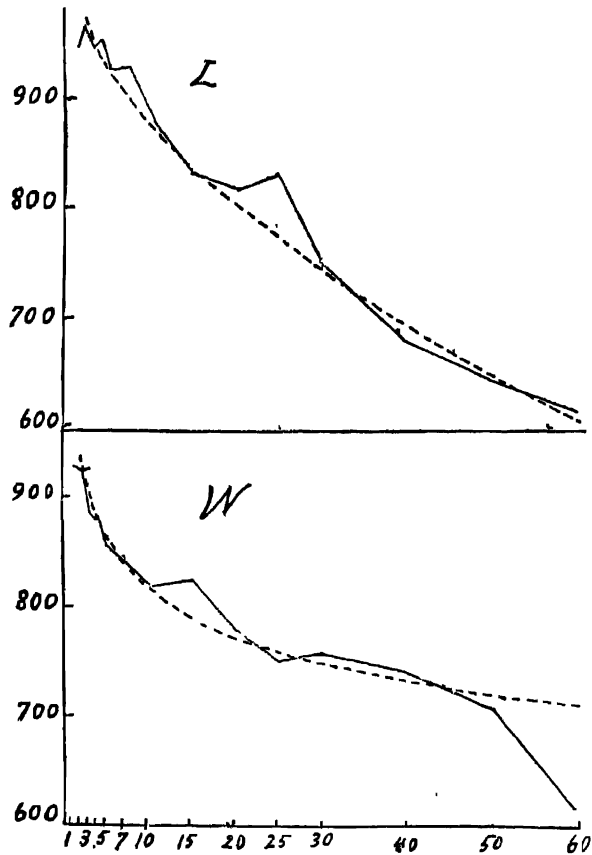


Fig. 45. CURVES FOR TONE MEMORY.

The law of memory for tones under these circumstances has been stated by Wolfe to be $\frac{r}{f} = \frac{k}{\log t} + c$

where r indicates the number of correct answers, f the number of wrong answers, t the interval of time elapsed and k and c two constants depending upon the particular individual and the circumstances of the experiment.

For example, if for the subject L we put $k = 12$, $c = -5.2$, and if for W we put $k = 4.43$, $c = 0$ we get series of values that correspond well to the actual results

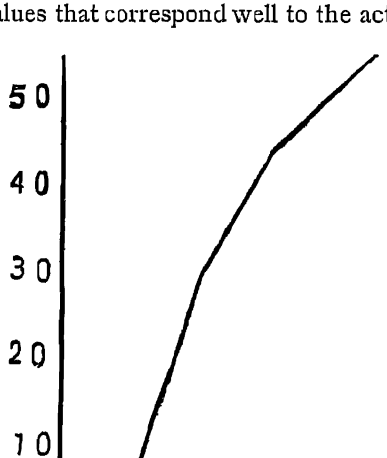


Fig. 46. LAW OF REPETITION IN MEMORISING.

obtained. These series of values are indicated by the dotted lines in Fig. 45; the agreement with the actual results is fairly satisfactory. We can thus accept the above formula as the law for tone memory.

Experiments with nonsense syllables, as already mentioned in Chap. II., have been made by Ebbinghaus.[1] A number of syllables, about 2,300, were formed with a

[1] Ebbinghaus, "Ueber d. Gedächtniss," Leipzig, 1885

vowel between two consonants, *e.g.*, dob, fik, &c. These were mixed together, and were picked up just as they happened to come. A certain number of these syllables formed a series which was learned by repeating them; the series was considered to be learned when it could be given without mistake or hesitation from beginning to end at a regular rate.

The first problem settled was the dependence of the number of repetitions on the length of the series. The results are shown in Fig. 46. For series up to seven syllables only one repetition is necessary; after that the amount of work increases with great rapidity for each additional syllable.

The next problem was concerned with the dependence of the amount remembered on the number of repetitions of the original series. Each series consisted of sixteen syllables, and was repeated 8, 16, ..., 64 times. After 24 hours the series was repeated until learned again. The rate at which the syllables were repeated was 150 to the minute. The results are shown in the following table :—

After x repetitions	the series was learned 24 hours later in y seconds.	Amount saved in seconds.	Saving per syllable for each repetition.
x =	y =		
0	1270		
8	1167	103	12.9
16	1078	192	12.0
24	975	295	12.3
32	863	407	12.7
42	697	573	13.6
53	585	685	12.9
64	454	816	12.8

Thus when such a series had not been seen before it took 1,270^s to learn them. When they had been repeated 8 times it took 1,167^s to relearn them, &c. A

repetition of 8 times in the first place saved 103" in relearning them, or 12.9" per syllable. That is, under these particular conditions the memory effect increased proportionally to the number of repetitions.

The third problem related to the dependence of the memory effect on the length of the elapsed interval. A series of thirteen syllables was repeated until it could be given without mistake. It was then laid aside for a definite time t, at the end of which it was again learned. Any memory effect would show itself in a decrease of the necessary number of repetitions in the second case as compared with the first one. The results for various intervals were as follows :—

After x hours	so much was still retained that in relearning there was a saving of y per cent. of the original labour.	The amount forgotten was equivalent to v per cent. of the original labour.
x=	y=	v=
0.33	58.2	41.8
1.	44.2	55.8
8.8	35.8	64.2
24.	33.7	66.3
48.	27.8	72 2
6 × 24	25.4	74.6
31 × 24	21.1	78.9

It is evident that the rate at which we forget things is at first very rapid until we finally come down to a small residuum that persists with great constancy. Ebbinghaus's results agree well with the formula $\frac{b}{v}=\frac{k}{(\log t)\,c}$ where b denotes the amount retained and v the amount lost, while k and c are two constants depending upon peculiarities of the observer and the conditions of the experiments. In both Ebbinghaus's and Wolfe's experiments we find a peculiar logarithmic form for the law of memory.

A final problem is that of the repeated learning of the syllables. At intervals of just one day after the first learning the series was repeated until on each occasion it was relearned. For example, a series of twelve syllables required 16.5 repetitions for the first learning; 24 hours afterwards it required 11 repetitions to relearn it; 24 hours after this relearning it required only 7.5 repetitions, &c. The results of Ebbinghaus's experiments were as follows :—

Number of syllables in a series.	Number of repetitions necessary for relearning the series in successive days.					
	I.	II.	III.	IV.	V.	VI.
12	16.5	11	7.5	5	3	2.5
24	44	22.5	12.5	7.5	4.5	3.5
36	55	23	11	7.5	4.5	3.5
1 stanza of "Don Juan."	7.75	3.75	1.75	0.5	0	0

Müller and Schumann,[1] with more elaborate methods, extended the investigation of memory to cover various problems, such as the influence of rhythm, the available quantity of attention-energy, the influence of practice, &c. For example, with three subjects (Germans) a series of syllables was learned more quickly if repeated in trochee — ⏑ than if repeated in iambus ⏑ —. This may arise from the general character of the German language, which is trochaic. Again, the second syllable of a trochaic or iambic measure has a tendency to recall the first syllable of the

[1] Müller and Schumann, *Experimentelle Beiträge z. Untersuchung des Gedächtnisses*, "Zt. f. Pysch. u. Phys. d. Sinn.," 1893, vi. 18, 257.

same measure rather than that of the following measure. Series learned in trochee require more work if relearned in iambic, and likewise the reverse.

I must here close the consideration of time-influence. Concerning one of its laws, $U = F(t)$, we can conclude that it has a logarithmic form. Concerning the other law, $C = f(t)$, and concerning the various modifications of both laws under different circumstances, we are still practically in ignorance.

CHAPTER XIII.

SUCCESSION IN TIME.

THE investigations on time-influence, or memory, lead us to the problem of what causes an idea to be reproduced. Suppose, for example, at a certain moment t_0 I see a tulip and at a later moment t_1 a memory of this tulip occurs to me; what produced it in the second case? There are three factors to be considered—(1) the original idea; (2) the state of mind immediately preceding the moment t_1; (3) the past history of the individual.

The first factor has remained almost entirely uninvestigated experimentally. The only question that has been formulated concerns the relative superiority of visual, auditory, or motor memories, or their combinations. The experiments have been carried out by methods that hardly satisfy the demands of the problem, and with different observers have led to conflicting and irregular results. The effort has been made to investigate the effects of vividness, repetition, &c., of the original idea.[1] The results confirm our general experience that memory is aided by vividness, repetition, recentness, &c. The various other questions concerning the qualities of the original idea can be answered only in terms of ordinary experience, and

[1] Calkins, *Association*, "Psych. Rev.," 1896; Monograph Supplement, No. 2.

remain uninvestigated scientifically, although a considerable amount of practical knowledge has been accumulated for educational purposes.[1]

The second factor—namely, the state of mind immediately preceding the revival of the idea—has received considerable attention under the name of association of ideas. In the first place, how are ideas associated; or, in other words, what is the relation between an idea and the one that is brought forward in connection with it? The answer cannot at present go beyond the qualitative stage.

A convenient method of presenting visual ideas is to have them placed as objects, pictures, or words in front of a photographic lens, with a shutter having a pneumatic release. The subject is placed so that he has before him a plate of ground glass like the focusing plate of a camera. When the shutter is released, he sees in front of him the object on the ground glass. Auditory ideas are most conveniently presented by leaving the subject in darkness and simply speaking the word or producing the sound. Objects can be touched, tasted, &c., in a similar fashion.

An investigation[2] was carried out by these methods with the purpose of determining, without prejudice from or relation to the Aristotelian "laws of association," just what really took place in an association of ideas.

In the first place, it was noticed that the primary, or inducing, idea generally underwent some transformation before an association took place. One change con-

[1] A brief consideration of some methods of improving memory will be found in Scripture, "Thinking, Feeling, Doing," 247, Meadville, 1895.

[2] Scripture, *Ueber den associativen Verlauf der Vorstellungen*, Diss., Leipzig, 1891; also in "Phil. Stud.," 1891, vii. 50.

sisted in the loss of some part. For example, the word FLUCH was followed by, or perhaps we should say was changed to, the word FLUSH. This change can be indicated by the scheme :—

a_1	a_1
a_2	a_2
a_3	a_3
a_4	b
a_5	a_5

In another case the inducing idea was a feeling of roughness derived by touching the fingers to some blotting paper. This was followed by the touch-idea, rough paper, and this in turn by the visual-idea, brown paper. This would be represented by the following scheme :—

a (rough)	a	b
	b (paper)	c (brown)

In other cases the inducing idea apparently underwent no change, and the induced idea consisted in something added to it. For example, BIG becomes BIGGER, or a sound became the sound of a certain bell. In still other cases the inducing idea entirely disappeared. For example, the word STAND was followed by a visual memory of the theatre, because, as the subject explained, he generally stands at the theatre. Examples were found of all intermediate stages.

The next fact noticed in the investigation was a difference between two methods in which the induced idea was connected with the inducing idea. The two may be connected directly or indirectly by means of a third idea. For example, a taste of lemon-juice is followed by the word "lemon"; this would be a direct connection. A case where the colour, red, is followed by a

somewhat indefinite memory of strontium light, and this in turn by a scene from an opera, is a series of direct connections where the middle idea is not fully clear.

From this it is but a succession of steps through continually more indefinite and unnoticed ideas to those that are completely unconscious. This is what is meant by indirect connection, or mediate association.

Sir Wm. Hamilton[1] first called attention to such associations :—

"Suppose, for instance, that A, B, C, are three thoughts—that A and C cannot immediately suggest each other, but that each is associated with B, so that A will naturally suggest B, and B naturally suggest C. Now it may happen that we are conscious of A, and immediately thereafter of C. How is the anomaly to be explained? It can only be explained on the principle of latent modifications. A suggests C, not immediately, but through B; but as B . . . does not rise into consciousness, we are apt to consider it as non-existent . . . Thinking of Ben Lomond, this thought was immediately followed by the thought of the Prussian system of education. Now, conceivable connection between these two ideas there was none. A little reflection, however, explained the anomaly. On my last visit to the mountain, I had met upon its summit a German gentleman, and though I had no consciousness of the intermediate and unawakened links between Ben Lomond and the Prussian schools, they were undoubtedly there —the German—Germany—Prussia—and, these media being admitted, the connection between the extremes was manifest."

The question of the possibility of mediate association arose during my experiments on the course of ideas,

[1] Hamilton, "Lectures on Metaphysics," lect. xviii., vol. i. 352, Lond. and Edin., 1859.

and, not knowing of Hamilton's observation, I made an attempt to answer it.

In order to investigate the subject experimentally, the following method was devised.[1] On one card there was a German word and some Japanese characters. On another card there was a strange word (Japanese, in Roman letters), with the same characters. A series of cards, comprising half of each kind, was shown in irregular order. For example, in one experiment the following series (the Japanese characters being represented here by Greek letters) was shown in the order here given :—(1) HANA $\alpha\beta$, (2) HITO $\gamma\delta$, (3) IUKU $\varepsilon\varsigma$, (4) KURU $\eta\theta$, (5) MENSCH $\gamma\delta$, (6) GEHEN $\varepsilon\varsigma$, (7) KOMMEN $\eta\theta$, (8) BLUME $\alpha\beta$.

The subject was asked to state if he had noticed any associations between the first four words and the second four ; he had not. Thereupon the words alone without the characters were shown him, with the request to state the first thing that entered his mind after each. The results were as follows :—(1) HITO——MENSCH, (2) KURU——KOMMEN, (3) HANA—— ? (4) IUKU ——GEGEN, (5) KOMMEN——IUKU, (6) GEHEN —— ? (7) MENSCH——HITO, (8) BLUME—— HANA.

At the end the subject declared that all the associations were involuntary, that he could give no reason for the associations, and that the Japanese characters had not occurred to him at all. Several of these associations were, nevertheless, correct ; it seems probable that they were brought about by the influence of the Japanese characters which, nevertheless, had not entered into consciousness. This probability is increased by other experiments in which the word-association was cor-

[1] Scripture, *Ueber d. assoc. Verl. d. Vorst.*, "Phil. Stud.," 1891, vii. 81.

rectly made and was *followed* by the occurrence of the characters. In still other cases the association was correctly formed without thought of the characters, whereas the subject could reproduce them when asked. Finally, the characters themselves were found to be in all stages of indefiniteness and forgetfulness, even in correct associations. It is to be remembered that all experiments were rejected in which any associations were made between the two parts of the series while they were being shown.[1]

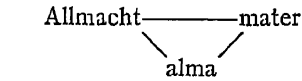
As this was the first attempt to investigate mediate association by means of experiment, the method was necessarily very crude; more striking results are to be looked for whenever a better form of experiment can be devised.

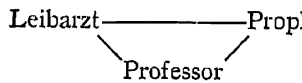
The problem has been made the subject of further investigation by Aschaffenburg.[2] In his experiments on the association of ideas, a number of cases occurred in which the connection between the two ideas was intelligible only on the supposition of an intermediate idea. In the greatest number of cases this intermediate idea appears to have been a sound-association with the inducing idea, while the relation of the induced idea to the intermediate one was of any kind. The following cases are some of many that occurred where there was absolutely no relation between the two associations; the supposed intermediate idea, which makes the association

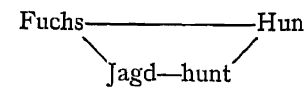
[1] Several other investigators have failed to find cases of mediate association: Münsterberg, "Beiträge z. exper. Psychol.," iv. 1 Howe, "Am. Jour. Psych.," 1894, vi. 239; Smith, "Zur Frage von der mittelbaren Assoc.," Diss., Leipzig, 1894.

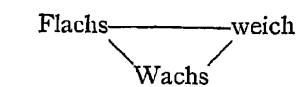
[2] Aschaffenburg, "Experimentelle Studien über Associationen, I. Theil, Leipzig, 1895. See also Thomas, *Ein weiteres Beispiel von Association durch eine Geruchempfindung*, "Zt. f. Psych. u. Phys. d Sinn.," 1895, xii. 60.

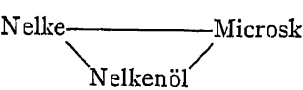
clear is placed below (Aschaffenburg's examples are, of course, in the German language).

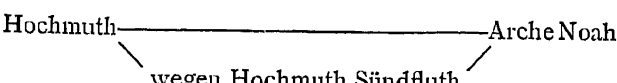




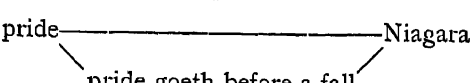


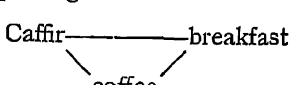


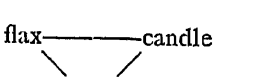




Similar examples in English would be :—







The intermediate idea in most cases did not remain completely unconscious, but appeared as a following

association. To illustrate by a personal experience. On one occasion, to the word "shoe" I associated the phrase "baby's shoes," and immediately thereafter I realised a dim idea which I had felt to be the cause of the association, namely, a kindergarten scene in which the phrase of a song "baby's shoes" stood out most prominently.

There are some further investigations that have bearings on the fact of mediate association.

It has been experimentally proved[1] that an idea can be brought forward by an association with something of which the subject is not conscious.

Cards were prepared which contained a picture in the middle, and a small letter or character in one corner. A series of four or five such cards was flashed in succession on the ground glass of the apparatus described above. The time of exposure was made so short that, at the most, the subject was able to recognise only the picture without the small letter. Thereafter the small characters were exhibited alone, and the subject had to state which of the pictures first occurred to him. The following is a specimen series, the pictures being indicated by words.

Peacock. F.	Shield. A.	Cat. I.	Flag. : :	Negro. C.

The results on one occasion were I—cat, : :—flag, A—shield, C—negro, F—?.

Upon being questioned, the observer stated that he had not recognised any of the characters in the original series; he consequently had no reason to

[1] Scripture, *Ueber d. assoc. Verl. d. Vorst.*, "Phil. Stud.," 1891, vii. 136; see also p. 391.

give for his associations. In some cases the subject would feel that a certain picture belonged to a certain letter, although he had not seen the letter before, as far as his knowledge went. From a number of experiments of this kind, I feel justified in drawing the conclusion that an idea which has not entered into the subject's full consciousness can be associated to an idea in consciousness, and can serve to reproduce it on a future occasion. The completion of the experiment—not yet attempted—would lie in exposing the character alone for too brief a time to be seen, and noting whether it then would be able to reproduce the picture to which it was associated.

A case of such association through unnoticed ideas is reported by Jerusalem.[1] A forgotten scene of three years past was suddenly remembered, apparently without the slightest cause. After persistent search the connection was found in the presence of an unnoticed odour from a flower, with which the subject had first made acquaintance on the occasion of the scene. Wundt[2] points out that the inducing idea must have been in consciousness, although not noticed or perceived.

Both Aschaffenburg and I have found these intermediate ideas in all degrees of consciousness, from full consciousness, in which the succession appears as a series of three ideas, down to complete unconsciousness, where the idea is completely forgotten, or is not even recognised when shown.

Of interest in this connection are some experiments that show the influence of forgotten associations.

[1] Jerusalem, *Ein Beispiel von Association durch unbewusste Mittelglieder*, "Phil. Stud.," 1894, x. 323.

[2] Wundt, *Sind die Mittelglieder einer mittelbaren Association bewusst oder unbewusst?* "Phil. Stud.," 1894, x. 326.

Kraepelin[1] used on seventeen successive days the same series of inducing ideas, and measured the association-time. This time decreased during the first few days, and then remained at a constant level of about one half the original time. After an interval of one and three-quarter years the same ideas were used among others in measurements of association-time; for these ideas the association-time was much shorter than for the others, although the earlier associations had been forgotten.

In conclusion, we can say that the fact of the existence of mediate association can be considered as proven, although these associations do not occur oftener than —according to Aschaffenburg—about four times out of a hundred. Aschaffenburg has also found that the average association-time for these cases is longer than for ordinary associations.

Turning to the third factor of association, namely, the past experience of the individual, we find the experimental data to be extremely limited. Galton[2] counted the number of associations derived originally from childhood and early youth, from manhood, and from the immediate past. In his particular case most of them were derived from the period of manhood, and least of them from the immediate past. On several occasions groups of students have been asked to write down a number of words as rapidly as possible. Various results have been obtained. On one occasion the women were found to use proportionately more words referring to wearing apparel, furnishings, foods, &c., whereas the

[1] Kraepelin, *Ueber den Einfluss der Uebung auf die Dauer von Associationen*, "St. Petersburg. med. Wochenschrift," 1889, No. 1 (cited from Aschaffenburg).

[2] Galton, *Psychometric Experiments*, "Nineteenth Century," 1879; "Brain," 1879, ii. 149; and "Inquiries into Human Faculty," 185.

men most frequently wrote words referring to the animal kingdom, verbs, implements, &c. On another occasion it was found that the women most frequently used abstract terms, adjectives, words referring to the animal kingdom, to educational matters, &c.[1]

It cannot be said that definite conclusions have been reached by these tests. Beyond this, our knowledge ends with commonplaces, such as: persons will most frequently associate ideas from their particular circles of interest, familiar ideas will occur more frequently than unfamiliar ones, &c.

In conclusion, we may say that the problem of the association of ideas has not been solved. The old "laws of association" by similarity, contrast, contiguity in space, and succession in time, are mere schemes for classifying associations. The real law of association which shall express the probability for the recall of a certain idea as dependent on its original intensity, on the past life of the individual, and on the present circumstances, has never been found.

[1] Nevers, *Dr. Jastrow on Community of Ideas of Men and Women*, "Psychol. Rev," 1895, ii 363.

PART III.

ENERGY.

CHAPTER XIV.

STANDARDS OF ENERGY.

Suppose we are about to grind the coffee for breakfast. In the first place, we test the empty mill by a few turns of the crank; the effort was hardly worth noticing. Putting some coffee in the mill we proceed to the grinding. Considerable effort is required; if kept up long we become fatigued. The result of the effort is that a number of ounces of coffee have been changed from beans to particles; we have done some *work*.

If we turn the mill rapidly we feel that we are making more exertion; but we are rewarded by the extra amount of coffee ground per minute. Both the effort, or work required, and the result, or the work accomplished, are greater. If we turn the mill slowly the effort is less, and the result is smaller.

If we adjust the mill to grind coarsely, an ounce of coffee goes through the mill with less effort than if we had adjusted the mill to grind finely. It takes less work to grind one ounce coarsely than to grind one ounce finely. To regrind the coarse coffee into finer coffee we must perform just about enough additional

work to make the total of the two grindings from bean to coarse and coarse to fine equal to one grinding from bean to fine. Fine coffee evidently represented more work than coarse coffee. To obtain ground coffee from the bean we must *work* for it. The ability to do this work is called *energy*.

Suppose that we grind coffee during the time of one minute. If we can put forth a very strong effort we can grind a great deal more than if we can make only a weak effort. The amount of work done depends on the intensity of our ability to do the work. One of the factors of energy is its *intensity*.

Take the case of a store where the coffee is ground by machine power. We will suppose that the machine can grind s kilos. of coffee per minute to a certain grade of fineness. The amount of coffee-grinding energy in our store is s kilos. per minute. If a second machine is placed in the store the amount of available energy is $2s$ kilos. per minute. If the store can accommodate r machines, the amount is $r.s$ kilos. per minute. The number of kilos. s for each machine has remained the same; the intensity-factor of the units of coffee-grinding energy has remained constant; but the amount of energy depended also on the *capacity* of the store to hold these units of intensity. Energy thus has two factors, its intensity and its capacity.[1] In general $E=ci$ where E is the amount of energy, c the capacity for energy in a given case, and i the intensity of the energy per unit of capacity.

While we are grinding coffee we are producing work, but as time passes we feel fatigued, till finally we can work no longer. We have performed a certain amount of work. Our ability to perform this amount of work

[1] Gibbs, *On the Equilibrium of Hetereogeneous Substances*, "Trans. Conn. Acad.," 1875, iii. 108, 343.

was, before starting, the amount of energy we possessed. As we performed more and more of the work, the ability for work, or the amount of energy, left became less.

In the following chapters we shall especially consider the intensity-factor, this being what is meant by the usual expressions : intensity of effort, intensity of sensation, &c. The capacity-factor has been subjected to little investigation.

Energy is known to us under various forms, of which the most prominent are energy of movement, energy of space, energy of warmth, energy of light, &c.

Energy passes from one form to another. Suppose a ball to be thrown against a wall. There was originally mental energy in the form of effort, then energy of movement in the arm, then energy of movement in the flying ball, and finally energy of warmth in the surface struck by the ball. The amount of energy in any case is measured by the quantity of work that can be performed. The work done may lie in changing the position of an object, in producing heat, in electricity, in chemical combination, &c. The end of the process in any case is a capacity for more work. If an object is raised upward by work, its position is really an ability for more work to be done by falling. If heat is produced, this very heat can do work in its turn. Energy in a closed system can, according to modern theories, never be lost. If it performs work, the result is the production of an equal quantity of energy of the same or of a different kind. Among the different kinds of energy the form to which we try to reduce all others is mechanical energy. We therefore need a standard of mechanical energy.

With the maximum energy we are capable of, let us start various balls in succession rolling along the bowling-alley. Some balls will go slowly, some swiftly. If the

balls are all of the same material, the big ones will go slowly and the little ones swiftly. If they are of different materials, the leaden ones will go more slowly than the wooden ones. Since the same quantity of energy was put into each, there must be a relation between the swiftness of movement and the kind of ball. The swiftness of the movement can be regarded as the intensity of the energy present in the moving ball. If with the same total energy there is a difference between the different balls, the balls must have different capacities for energy. This capacity of an object for energy is termed its *mass*. Leaden balls consume more energy than wooden balls of the same size, and their mass is said to be greater.

As the standard of mass the *kilogramme des Archives* is assumed, and a scale is formed from its multiples or sub-multiples. The unit of mass for scientific work is the thousandth part of a kilo., or a gramme.

It is, perhaps, hardly needful to call attention to the fact that by "mass" we mean no reference to matter. As Ostwald puts it: "Since the factors of energy, which are proportional to one another, such as mass, weight, volume, capacity for heat, and capacity for chemical energy, always appear bound together at some point in space, we have adopted the habit of considering them all as contained in a bearer or vessel of energy to which we give the name 'matter.' In reality we experience nothing of this so-called matter but the energy-factors mentioned. When we note that these always appear without separation in space, we have given the total result which the hypothesis presents to us of a bearer of energy different from the energy itself. It seems superfluous to set up a special hypothesis for so simple a fact; it is also not to be denied that it has worked as a great hindrance to the formation of clear concepts concerning the character of energy. 'Matter' is thus

nothing but a sum of energy-factors not separated in space. Those factors of energy that are proportional to one another and to mass, we are accustomed to name the fundamental properties of matter, whereby we give preference to the mechanical ones (mass, weight, 'impenetrability,' or volume), although others, *e.g.*, the susceptibility to chemical changes, are no less properties of all matter than the others. The other factors of energy that are not necessarily proportional to the former, such as velocity, temperature, electric potential, &c., we are accustomed to name temporary attributes or conditions of matter." [1] We can therefore dismiss at once all notions of bodies except as aggregates of energy.

Is the standard of energy a physical or a psychological one? Just as in the case of time, the establishment of standards of energy is made on the basis of our mental experience. By an effort, by the exertion of force, we push and pull objects about; we thus derive our notions of bodies as exerting forces on one another. In lifting a weight we feel the force of gravity; in stopping a flying ball we feel the work of resistance. Modern mechanics defines force in terms of mass and acceleration, *i.e.*, the movement of a given mass through a given distance in a given time. In this way we regard it only as an unknown factor related to motion; but this abstraction does not mean anything to us mentally till we imagine some muscular force behind it.

The standard of energy is thus both a physical and a psychological one. It is physical because it is ultimately established by instrumental means; it is psychological because no step of the process goes outside of our experience.

[1] Ostwald, *Studien zur Energetik*, "Zeitschr. f. phys. Chemie," 1892, x. 375.

The force of gravity is the factor that manifests itself in the acceleration of a falling body. When holding a ball in the hand, we counteract the force of gravity by a muscular exertion. If we let the ball fall, it falls at an accelerated speed. The acceleration of the ball is taken as the measure of the force of gravity; it can also be used, with proper regard to the weight of the arm, &c., as the measure of the muscular exertion.

Proceeding just as in the case of time, we improve our methods of testing and comparing falling bodies by introducing other senses, apparatus, &c., till by careful elimination of sources of variation we come to a result that gives the maximum of agreement for the measurement of force.

The generally adopted unit is the force which, acting upon a gramme for one second, produces a velocity of one centimetre per second.

In mechanics the unit of mass and the units of time and space are used as the fundamental units; the unit of energy is treated as a derived unit.

The unit of mechanical energy is the erg, or the amount of work done by a unit force acting through a centimetre; or, it is the amount of energy contained in a body of 1 g. moving through a distance of 1cm. in 1". It is the amount of work which would be required to generate the motion of the body, or is the amount of work which the body would perform if stopped.

Practically, the difference between using time, space, and mass, and using time, space, and energy as fundamental units is of minor importance; theoretically, however, any intelligible treatment of mental life must start from energy as a prime factor.

CHAPTER XV.

ENERGY OF VOLUNTARY ACTION.

WHEN we press the thumb and index finger on the dynamometer (Fig. 4), we are more or less distinctly conscious of the intensity with which it is done. We can at any rate be fully conscious of the energy with which we intended it to be done; we can intend to make two successive pressures alike or different; we can intend to make one of them twice or three times as energetic as the other.

The first problem in dynamometry is to investigate our scale of voluntary action. We will suppose for the present that the marks on the scale of the dynamometer are in millimetres or any other arbitrary units. You have the dynamometer between your thumb and finger (eyes closed), and I tell you to produce a momentary light pressure of any strength you desire. You do so, and I privately record the result. Now I tell you to produce a pressure twice as strong. You do so to the best of your ability. I again record the result. Likewise we obtain pressures three, four, &c., times as strong as the first. Here we have, in the first place, a definite intention in each case to produce a pressure and a definite resulting pressure recorded in arbitrary units. The definite intention we can call by the usual name, volition.

For psychological purposes we do not need to inquire into the physical and anatomical processes underlying

the execution of the volition. We may be totally ignorant of the laws of elasticity for the dynamometer-rods, but we know by experiment that the application of standard weights to the rubber button of the dynamometer produces certain deflections, and thus we get a scale of weights. We may be quite in doubt as to the parts played by muscular sensitiveness, the skin, and the joints in arousing the peripheral nerves; we are absolutely in ignorance concerning the further processes in the nervous system; and even if we did know all about them, it is very doubtful if the knowledge would in the least affect our volitions and their results. Consequently, in the present investigation, we are concerned only with the deflections of the dynamometer in response to volitions of different intensity and with the deflections in response to weights placed upon it. By means of these latter deflections we establish a standard scale for voluntary energy.

In order to establish a standard scale of voluntary energy we must proceed, in the manner previously explained, to apply the rule of $1 + 1 = 2$. A volition of any desired energy is produced and the result is recorded on any convenient form of scale, let us say an angle-scale of a dynamometer. Then, as we know metal weights to be more constant than our volitional efforts, we obtain a weight that produces the same angular deflection. Then we take two such weights and apply them to the dynamometer, then three, &c., with the result that the angle-scale can be changed to a weight-scale. This weight-scale is then adopted as the standard. With this standard we then compare our particular scale of volition-energy.

The graduation of the dynamometer in grammes is a purely arbitrary affair. We might have used angular deflections, or millimetres of movement of the point,

We used grammes because in supporting objects, *e.g.*, by the outstretched finger, much (but not quite) the same volitions come into play, and because the gramme is the unit of energy for that particular case. The arbitrariness of the gramme-scale can be seen from the fact that of the actual energy expended by the muscles about three-quarters go to produce heat and one-quarter to produce the pressure. We might with just as much right, and perhaps with just as much success, use the rise in the temperature of the muscle as the preliminary scale. Again, the work done by the muscles need not necessarily be proportional to that arriving along the peripheral nerves, or starting from the brain. The point to be borne in mind is that, following the volition, there is a whole series of processes ending in the movement of the pointer over a scale-plate, and that not one of these processes bears the remotest resemblance to the original volition. The graduation in grammes has advantages over any other for the dynamometer, and it is used for convenience. A man whose hand has been amputated can perform and apparently (for him) execute the same volitions. For him some other apparatus and preliminary scale would be necessary, possibly a pair of electrodes on the arm-stump with a determination of the change of electric potential. If the experiment could be successfully carried out, this scale would be just as appropriate as the gramme-scale of the dynamometer.

In any case, the scale we are seeking is the volition-scale for the particular mental phenomenon of pressure. This pressure has no resemblance whatever to the work done by a weight mechanically deflecting the dynamometer. There is really no "pressure" of the weight on the dynamometer; there is only a change in the distribution of energy. The application of the term "pressure" to this case arises from the fact that in most cases of

pressure, as the execution of a volition, we find results happening similar to those in cases where weights are applied. If a person should grow up with reading telescopes (for a galvanometer) fixed to his eyes and various electrodes to the surface of his body, "pressure" would mean to him introspectively the same thing as for all of us, but, referring to his visual experience, it would mean degrees of mirror deflection and not grammes.

We might mark the scale-plate of the dynamometer in volition-units, starting with the first light pressure as the unit. This would be preferable if all the experiments of the investigation were to be made on a single occasion, and if the investigation were not to be communicated to others. But if the experiments are carried out on different occasions or on different persons, they can be compared only by reference to some common scale. If the scale of the first person is used, it is just as arbitrary for the others as the gramme-scale. Finally, for communicating results reference must be had to some generally accepted scale and not to an arbitrary one.

This scale of volitions is not a standard scale, but is the actual scale present in us at the particular moment. It undoubtedly changes from time to time, and under various circumstances; its particular form is probably due to past experiences.

Experiments that I have made on establishing scales of volition in the thumb-and-finger grip, give the following results for three subjects:—

Volition Scale:	1	2	3	4
Subject I.	0.6	1.1	2.1	3.3 kilos.
" II.	0.4	1.2	2.1	4.3 "
" III.	0.6	1.1	1.9	3.9 "
Relative values I.	1	1.8	3.5	5.5
" " II.	1	3	5.3	13.8
" " III.	1	1.8	3.2	6.5

With a scale of volitions thus obtained, we naturally turn next to the various mental influences modifying our volitions.

Researches on dynamometry have been very numerous. The results I shall give here are not derived from any one investigation, and were not obtained with any one apparatus. The most common apparatus is a dynamometer for the whole grip of the hand, which is unfortunately not an accurate instrument. It is needless to go into apparatus details, however, as the experiments can all be repeated under even better conditions with the apparatus I have described.

In one important respect we find ourselves disappointed. The results of previous investigations are generally only qualitative, and not quantitative. Nevertheless, numerous interesting and valuable facts have been discovered. I will give some of the results briefly.

At the outset we notice that the investigations have been almost entirely confined to the study of the amount of the maximum effort that can be exerted; the problem almost always refers to the greatest possible grip of the hand. The object in most of these cases is to compare the maximum energy of different persons, *e.g.*, of the two sexes, of different races, &c.[1] Such measurements, though valuable for anthropological purposes, are seldom carried out in a way to throw any light on mental processes.

"The greatest possible effort depends on the general mental condition. It is greater on the average among the intelligent Europeans than among the Africans or Malays. It is greater for intelligent mechanics than for common labourers who work exclusively, but un-

[1] A summary of these results is to be found in Ellis, *Man and Woman*, 150, London, 1894.

intelligently, with the hands. Intellectual excitement increases the power. A lecturer actually becomes a stronger man as he steps on the platform. A schoolboy hits harder when his rival is on the same playground. A bear's fear for the safety of her cubs might well be considered proportional to the number of pounds difference in the force of her blow."

The amount of the greatest possible effort can be increased by practice.

Curiously enough, this increase of force is not confined to the particular act. In a set of experiments made to test this point,[1] the greatest possible effort in gripping was made on the first day with the left hand singly and then with the right hand, ten times each. The records were : for the left 15 lbs., for the right 15 lbs. Thereafter, the *right* hand alone was practised nearly every day for eleven days, while the left hand was not used. The right hand gained steadily day by day; on the twelfth day it recorded a grip of 25 lbs. The left hand recorded on the same day a grip of 21 lbs. Thus the left hand had gained 6 lbs., or more than one third, by practice of the other hand.

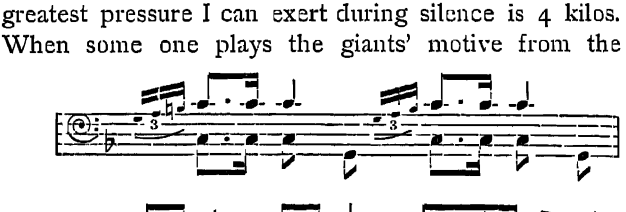
A great deal has been said of the relation of physical exercise to will power. I think that what I have said sufficiently explains how we can use the force of act as an index to will power. It is unquestionable that gymnastic exercise increases the force of act. The conclusion seems clear: the force of will for those particular acts must also be increased. It has often been noticed that an act will grow steadily stronger although not the slightest change can be found in the muscle.

Of course, I do not say that the developed muscle does not give a greater result for the same impulse than

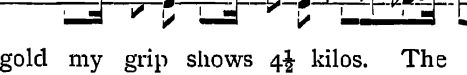
[1] Scripture, Smith, and Brown, *On the Education of Muscular Control and Power*, "Stud. Yale Psych. Lab.," 1895, ii. 118.

the undeveloped one; but I do claim that much of the increase or decrease of strength is due to a change in will power. If we consider the matter on the physiological side, this would be equivalent to the assertion that the change in the force of the act is due partly to a change in the amount of energy liberated by those higher nervous centres which are most immediately involved in the volition.

The force of will varies according to what we hear, feel, or see. With the thumb-and-finger grip the greatest pressure I can exert during silence is 4 kilos. When some one plays the giants' motive from the



Rheingold my grip shows $4\frac{1}{2}$ kilos. The slumber motive from the Walkürie reduces the power to $3\frac{1}{4}$ kilos.[1]



The effect of martial music on soldiers is well known. The Marseillaise helped to achieve the French Revolution.

Just how much of the effect is due to the rhythm, the time, the melody, and the harmony, has not been determined. A very great deal depends on the pitch. Plato emphasises the influence of the proper music on the formation of character, and proceeds

[1] Scripture, "Thinking, Feeling, Doing," p. 85, Meadville, 1895.

further to specify the general scales in which music should be written. The high Lydian is plaintive, the Ionian and Lydian are soft and convivial, the Dorian is the music of courage, and the Phrygian of temperance. Aristotle agrees in general but considers the Phrygian

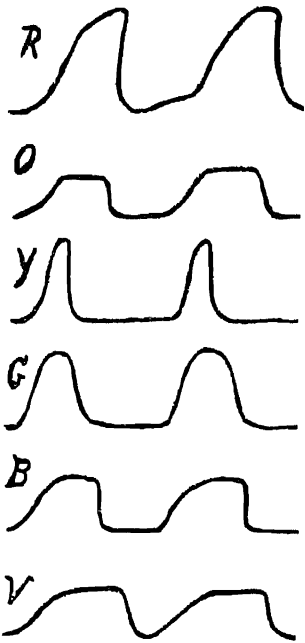


Fig. 47. DEPENDENCE OF ENERGY ON COLOUR

music as exciting and orgiastic. It has long been supposed that the difference among the scales was one of arrangement of the intervals within the octave, corresponding to the major and the minor, but a more recent opinion is that the difference is one of pitch. The Lydian is a tone to a tone and a half higher than

the Phrygian, and the Dorian is a tone below the Phrygian. The Dorian is neither too high nor too low, and expresses a manly character.

It might be suggested that the special melodies associated with each scale may have had much to do with the case. Nevertheless it has been proven that the pitch itself has an effect on the greatest strength of grip; tones of a moderate pitch increase the power of the grip whereas very high or very low tones weaken it.[1]

The sight of colours has been found by Féré to change the power of the grip, particularly in hysterical persons

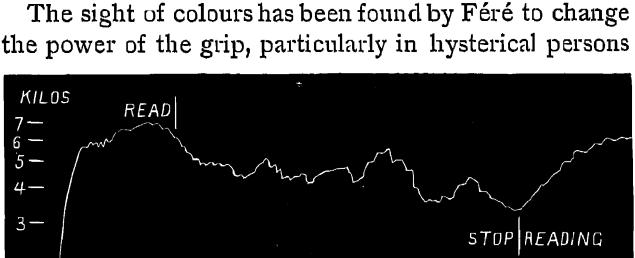


Fig. 48. DECREASE OF EFFORT OWING TO INTELLECTUAL WORK.

(Fig. 47). I have not been able to detect such differences in regard to colours in my experiments on college students; the experiments should be repeated with careful attention to the purity, brightness, and extent of the colours, the absence of sources of disturbance and distraction, &c.

Mental work requiring attention decreases the energy of any particular voluntary act. The following experiments[2] with a hand dynamometer show this effect; the

[1] Féré, "Sensation et mouvement," 35, Paris, 1887.

[2] Loeb, *Muskelthätigkeit als Maass psychischer Thätigkeit*, "Archiv f. d. ges. Physiologie" (Pflüger), 1886, xxxix. 592.

records of the dynamometer were made in a purely arbitrary scale of degrees. I give a few specimen results; the figure denotes the maximum pressure under the particular circumstances.

Experiment 1.—No distraction, 77°; reading and understanding, 15°; reading mechanically with no attention to the meaning, 67°.

Experiment 2.—No distraction, 76°; reckoning $6 \times 7 = 42$ (very little distraction), 74°.

Experiment 3.—No distraction, 104°; reckoning $13 \times 18 = 234$, 25°.

These figures, being purely arbitrary, give only a vague impression of the amount. With my dynamograph I have obtained tracings readable in kilos. Fig. 48 shows such a tracing; the scale of kilos. is at the left. The maximum of the thumb and finger grip is about 7 kilos.; the loss due to reading is about 3 kilos.

Turning from the experiments with the maximum energy to the study of other degrees of energy, we find only one extended research.[1]

The instrument used (Fig. 49[2]) consisted of a heavy spiral spring enclosed in the brass cylinder (R P), to which the handle (H) is attached by a bar. The bar runs on double wheels almost without friction. When the handle is pulled out the amount of force applied is shown on the scale (P), as in an ordinary spring balance. The pointer, however, not being attached to the bar, is only pushed forward, and stays at the point of the maximum pull. The experimenter can thus take the exact reading before replacing the pointer. By means of the bar, pivot, and screw (at N) the spring can be set

[1] Fullerton and Cattell, "On the Perception of Small Differences," 65, Philadelphia, 1892.

[2] Figs. 49 and 50 were kindly furnished by Professors Fullerton and Cattell.

at any point up to 15 kg. In such a case, the observer must pull the set amount before the handle moves, while the force of his pull beyond this amount is registered on the scale. The initial force of movement may be kept the same, while the extent and rate are altered, and the total extent of the movement may be made as small as desired.

The movement used was a free pull with the arm. The observer was told to give a pull of (say) 2 kg. As might have been expected, his error in estimating a standard magnitude was usually very great. He was

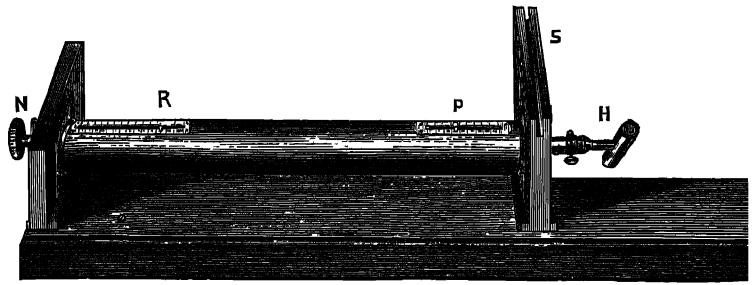


Fig. 49. DYNAMOMETER FOR PULLING.

then told the direction and approximate amount of his error and allowed to try again. This was repeated until he had made five trials, by which time he could usually give the standard without great variation. A series of ten judgments was then made, the observer giving in each trial first the standard pull from memory, and then a pull as nearly as possible equal to it.

The results show, in the first place, certain constant errors varying with the magnitude of the standard and with the particular observer. What we are particularly interested in, however, is the mean variation around the observer's own average. With standards of 2, 4, 8,

and 16 kilos, the mean variations for five observers were 0.19, 0.30, 0.43, and 0.46 kilos.

We have seen that when two sensations are compared the mean variation expresses the subject's irregularity of judgment, and that when movements are made it expresses his irregularity of execution. Cattell has shown the way to analyse the irregularity of actual execution into irregularity of movement and irregularity of sensation or perception. I will illustrate this by the case of one observer F. After each pair of intentionally equal pulls had been made, the observer was required to decide which of the two had actually been the

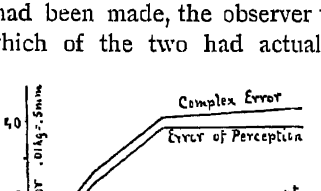


Fig. 50. IRREGULARITY DEPENDENT ON EFFORT.

greater. For a pull of 2 kilos. F felt correctly 71 % of the time when the second pull was really less, and 69 % of the time when it was really greater. He could thus himself often correctly distinguish a difference, even a difference between two pulls that he had intended to be equal. Similar results were found for all observers. The conclusion drawn by the authors is as follows: The error (irregularity) of execution is complex, being partly due to an error of perception and partly to an error of movement. If the entire error, in the attempt to make the two pulls alike, were due to an error of perception, the pulls would seem, when completed, exactly alike, and the observer's judgment would have been a mere guess, as likely to be wrong as right. If,

on the other hand, the observer did not make the two movements apparently alike, he should perceive his error of movement as a difference, and this difference would give a percentage of right cases corresponding to its size. Further, the average irregularity of perception, when used as the amount of difference in the method of right and wrong cases, gives 78.7 % of right cases. If the error of movement gave this percentage, we should conclude that the error of movement and the error of perception were equal. The percentage is regularly less than this amount; by the difference that corresponds to this percentage we determine the size of the error of perception as compared with the error of movement. For example, F gave 71 % of correct answers for the judgment of second pull for 2 kilos. If he had given 78.7 %, the ratio of his error of movement to that of perception would have been 1 to 1; for 71 %, however, it is as $\frac{7}{10}$ to 1.[1] The total error (mean variation) was 0.18 kilos.; how much of this belongs to perception, how much to movement? According to the law by which errors combine, the total error is the square root of the sum of the squares of the separate errors. Thus the errors bear the relation of 0.7 to 1.0; their squares are as 0.49 : 1.00. The total error will stand in the relation of $\sqrt{0.49 + 1.00}$, or 1.2, to the others. Thus the error of movement is $\frac{7}{12}$ of the total error, or 0.11 kilos.; and the error of perception is $\frac{10}{12}$ of 0.18 kilos., or 0.15 kilos. The general course of these errors for standard pulls of 2.00, 4.00, 8.00, and 16.00 kilos. is shown in Fig. 50.

[1] The values are found by the table in Appendix VIII.

CHAPTER XVI.

FATIGUE IN VOLUNTARY ACTION.

IN pressing the dynamometer we are performing work. As work is performed, our capacity for work decreases; this decrease is generally known by the term "fatigue."

Fatigue, like memory, introduces a change in the phenomenon itself and a change in its regularity. Fig. 51 shows a portion from a curve with the dynamograph

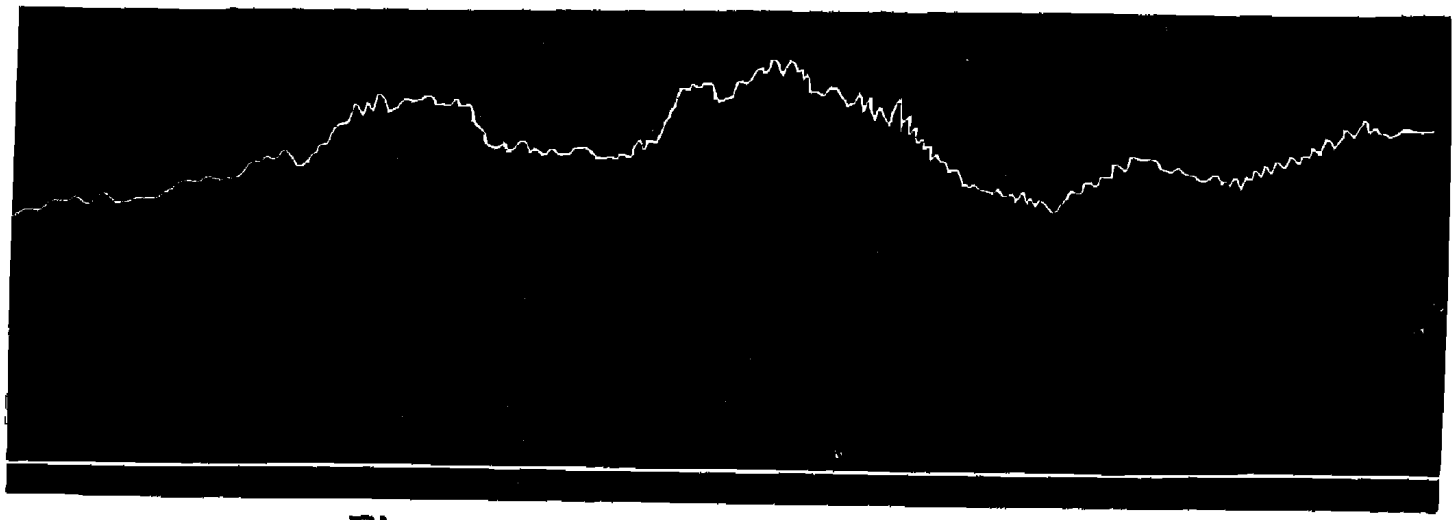


Fig. 51. FATIGUE IN CONSTANT EFFORT.

where the maximum grip was being exerted; it is the rear end of the same record as Fig. 48, a large portion from the middle of the record being omitted. The maximum grip exhibits large fluctuations, going from very low to even higher than the maximum at the start (Fig. 48). It is also to be noticed that the grip becomes very uncertain and tremulous.

Are these effects of fatigue due to bodily changes of

which we have no knowledge? or are they due to a change in will power?

Let us first change the method of experimenting. Instead of maintaining a constant pressure the subject is told to press on the dynamometer repeatedly as hard as he can and to continue to repeat these grips till told to stop. A record of such a series of grips, taken on myself, is shown in Fig. 52.

It will be noticed that the grips steadily decrease in extent to a condition of complete paralysis; then follows a partial recovery; then a paralysis. In longer records this fluctuation repeats itself continually. It

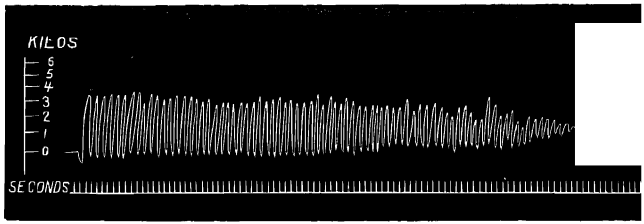


Fig. 52. FATIGUE IN REPEATED EFFORTS.

will also be noticed that the fluctuation occurs not only in gripping, but also in letting go. When the paralysis occurs, the grip does not sink to zero, but remains at a medium point. During the whole experiment I conscientiously gripped as hard as possible. Concerning the minor changes I knew nothing. At the points of paralysis I could not even *will* the grip; I felt a complete mental paralysis and did not suppose that I had gripped at all. On the other hand, I did not suppose that I had relaxed the grip. A kind of mental daze came over me at the points of paralysis; I felt absolutely unable to do anything with the fingers involved. I did not feel that I was willing the grip

as strongly as ever, but I felt a weakness in the very effort itself. This change in will-power became very marked when I was able to increase it after the time of paralysis had passed.

Returning now to the original method, that of maintaining a steady maximum grip (Fig. 51), I find analogous feelings of mental paralysis and daze, although the differences in the volitional effort are not so marked.

In the light of such experiences I do not hesitate to regard a portion of the fatigue as a fatigue in volitional energy for the particular movement involved.

This view is sustained by the experiments of Mosso and Lombard.

To measure the work done in muscular movements Mosso[1] has invented the ergograph. It consists of a rest in which the arm is fixed so that the middle finger can be moved alone without involving any of the others. The fingers to each side of the middle finger are kept in position by being inserted into tubes. A cord is attached to a ring on the first joint of the finger. This cord runs over a pulley and supports a weight at the end. Just behind the pulley arrangement a smoked drum is placed. A light pointer is attached to the cord; and each movement is thus recorded.

In Mosso's experiments the weight was raised as high as possible and then lowered every two seconds until complete fatigue occurred. This produced on the drum a series of tracings beginning with a long one and declining to zero at the end. Each of these tracings represents the amount of work done in raising one kilo. through a certain distance, and this distance represents the maximum energy present at the particular moment.

[1] Mosso, *Ueber die Gesetze der Ermüdung*, "Archiv f. Physiol." (Du Bois-Reymond), 1890, 89.

The curve according to which this energy decreases with successive efforts is the curve of fatigue. The

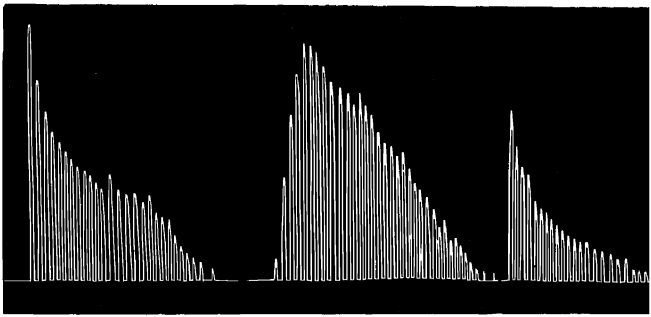


Fig. 53. ERGOGRAPH RECORDS FOR VOLUNTARY, NERVOUS AND MUSCULAR FATIGUE.

general course of this curve is exhibited by a line supposed to be drawn along the tops of the tracings.

The curve of fatigue differs according to the way in which the movement is originated. Fig. 53 shows: (1) a tracing when the movements were voluntary;

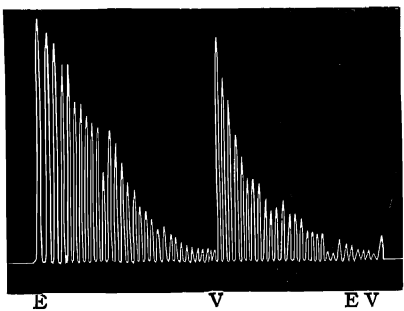


Fig. 54. RECORDS SHOWING INDEPENDENCE OF MUSCULAR AND VOLUNTARY FATIGUE.

(2) a tracing when the movements were produced by electrical stimulation of the nerve; and (3) a tracing

when they were produced by electrical stimulation of the muscle.

The actual amount of work done and the course of the curve of fatigue thus depend on both muscular, peripheral and mental, or central, factors. The curious interrelation between voluntary movements and those due to electrical stimulation of the nerve is exemplified in Fig. 54, where E indicates the movements due to successive electrical stimuli and V those due to voluntary efforts. After complete fatigue had occurred for electrical stimulation the person was at first quite fresh for the voluntary effort; he became, however, very

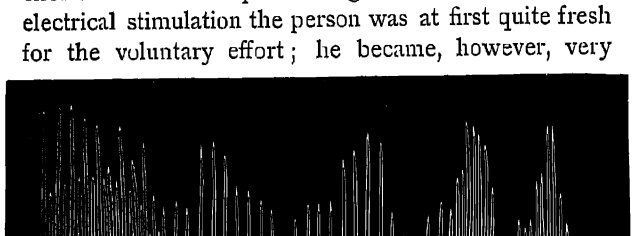


Fig. 55. REPEATED CONTRACTIONS.

rapidly fatigued. Thereafter there was a very slight recovery for each kind of movement.

The independence of voluntary and muscular fatigue has also been proven by Lombard[1] in the following way:—

He contracted the muscles alternately by a volition and by an electrical stimulus. The result is shown in Fig. 55.[2] The tops of the electrical contractions are

[1] Lombard, *Some of the Influences which affect the Power of Voluntary Muscular Contractions*, "Journal of Physiology," 1892, xiii. 1.

[2] This is a copy of an original record kindly furnished me by Prof. Lombard.

in general lower than the voluntary ones; they decrease very slowly in height. The others which are the results of the voluntary contractions are at first much higher; they decrease steadily until they are almost the same as the electrical ones. Just beyond this point there is a recovery of voluntary power, which lasts for a short time only, to fall and rise and fall again in constant succession.

It has thus been definitely proven by Lombard that the fluctuations in the voluntary efforts, such as are seen in Fig. 54, are not due to the muscles.[1]

We can conclude, therefore, that a considerable portion of the total fatigue in voluntary muscular contractions is a fatigue of will-power, and also that the fluctuations represent fluctuations in the effort of will. Turning again to Fig. 51, we can say that, while the average strength may decrease from causes unaffected by our will-power, the fluctuations of the curve represent fluctuations in effort of will.

The fatigue for a particular voluntary movement does not mean necessarily a fatigue for other voluntary acts. It is quite a mistake to regard the will as one single thing; there are as many wills as there are things to be willed. The fatigue of a particular kind of will does not involve a central will on which others depend.

There are, however, cases in which we use the fatigue of a particular form of will-power to indicate the general changes in the total sum of will-power in the individual under particular circumstances, just as we use the changes in a particular kind of sensation to indicate the changes in his general sensitiveness. We do not imply a

[1] Lombard, *The Effect of Fatigue on Voluntary Muscular Contractions*, "Am. Jour. Psych.," 1890, iii. 24; also *Effet de la fatigue sur la contraction musculaire voluntaire*, "Archives italienne de biologie," 1890, iii. 380.

central will-power in the first case any more than we do a central sensation-power in the other.

Among the subjects thus investigated we find the effect of general mental fatigue on the fatigue of movement. Of the curves shown in Fig. 56, the first shows the work done by the finger in Mosso's apparatus at nine o'clock in the morning by Dr. M. From 2 p.m. till 5.30 he was engaged in orally examining students under the most trying mental conditions. At 5.45 the second curve was taken; it shows the first contraction as strong as ever, but the voluntary energy is quickly exhausted.

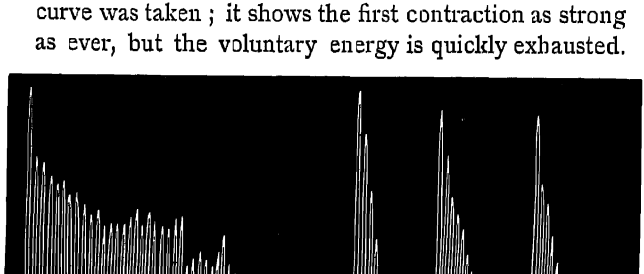


Fig. 56. EFFECT OF MENTAL WORK.

The third record was taken after supper at 7.30, and the fourth at nine o'clock. Of course, not all of the increased fatigue is central; similar curves with electrical stimulation show changes; they are, however, less marked. A part of the change is undoubtedly central, and is, according to our point of view, also psychical.

Mosso's explanation of the central fatigue as due to changes in the constitution of the blood or any other physiological explanations do not have any bearings on the psychological side of the question. We feel, mentally, that our voluntary power falls off more rapidly in the one case than in the other, and we assume that the

rate at which it falls off bears some relation to the actual curves obtained.

Lombard has investigated a number of the influences that affect volition-energy. In most of the cases we shall find that his results are confirmed by ordinary experience; others, however, are not so usual. The day's work leaves us with less energy than we start with in the morning. In Lombard's table[1] for each day of nearly a month, we find the percentages of loss for the evening: 59, 34, 39, 3, 11, 55, &c. Only on five days out of twenty-three is there a gain in the evening; this accords with the experience that occasionally one feels unusually fresh in the evening.

The same series of experiments shows the gains for every morning over the preceding evening; in only two cases was there a loss.

In general, Lombard found that the following influences decreased the power to do voluntary muscular work in his experiments: general and local fatigue, hunger, decreasing atmospheric pressure, high temperature, humidity, and tobacco. He found that the following increased it: exercise, rest, sleep, food, increasing atmospheric pressure, and alcohol.

In the experiments of Mosso and Lombard fatigue was produced rapidly by giving the finger a large amount of work to do. When such hard work is not required the fatigue is, of course, much less rapid and the work can be continued for a longer time. Experiments of this kind with the dynamometer have been carried out under my direction.[2] A dynamometer was used of the kind shown in Fig. 4, adjusted for very light grips. The subject started with any moderate grip he chose. This grip was then repeated with the same effort for

[1] Lombard, as before.

[2] Full tables will appear in "Stud. Yale Psych. Lab.," 1896, iv.

sixty times. Ten sets of experiments were taken. The results, as given in Fig. 57, show in the curve F the average increase for each successive grip over the first grip, and in the curve M the mean variation of the

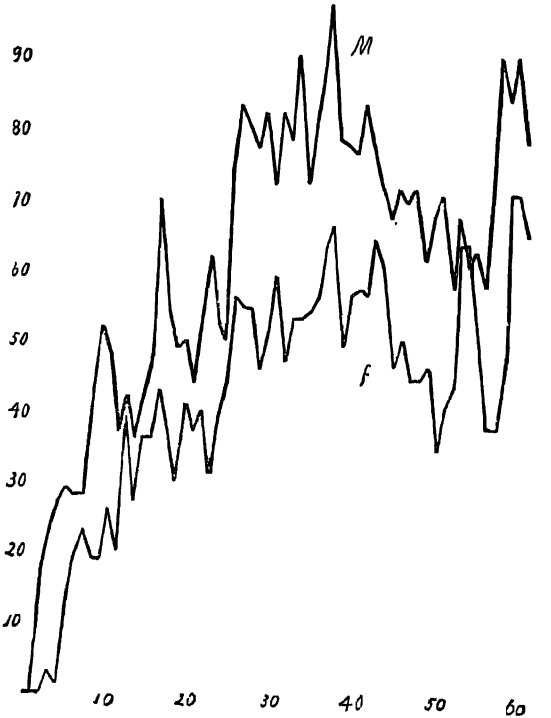


Fig. 57. INCREASE OF THE INACCURACY F AND THE UNCERTAINTY M IN A SUCCESSION OF EFFORTS.

successive grips. These quantities are analogous to the inaccuracy C and the uncertainty U that have already been discussed in Chap. XII.

Experiments of a similar kind have been made by

Moore.[1] The particular voluntary movements investigated were those of the eye muscles. The apparatus is shown in Fig. 58.

The cylindrical beads A and C were stationary at 1 m. apart; B was adjustable. As a bead was raised by the appropriate mechanism, it was seen by the subject looking through a slit in E.

The experiment began each time with the carriage of the second bead placed at the end of the slot nearest the observer. All beads were out of sight. By a push of the lever the first bead was raised, its position was noticed and it was allowed to fall. By the cord in the left hand the second bead was raised and dropped. Then by a pull of the lever the third was raised and dropped. The judgment was made as to whether or not the second bead was in the middle. If not, the middle bead was moved further away by turning a wheel and the experiment was repeated. This was done until the bead was judged to be in the middle.

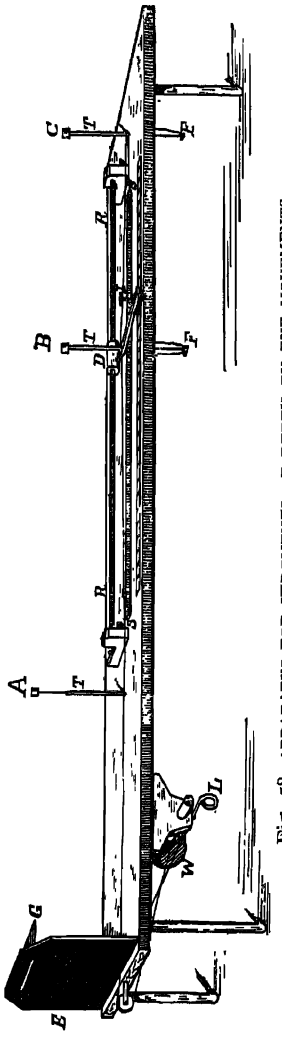


Fig. 58. APPARATUS FOR JUDGMENTS OF DEPTH BY EYE MOVEMENTS.

[1] Moore, *Studies of Fatigue*, "Stud. Yale Psych. Lab.," 1895, iii. 68.

The distance of the first bead from the eyes was a half metre; this was not short enough to cause any strain. The third bead was a metre and a half away. The part of the scale from the middle toward the eyes was called negative and the other positive; they were indicated by the signs − and +. Each individual experiment resulted in a position of the middle bead somewhere near the middle point, or o. Its divergence from this point was recorded in millimetres + or −.

The first question to be considered was the position of the estimated middle point for the first experiment on the successive days of work.

The results were: during May, 27, 60, 33, 28, 74, 97, 83, 96; during June, 75, 48, 48, 81, 80, 80, 90, 74, 61, 76, 86; during July, 92, 97, 102, 92, 103, 121, 136, 126, 70, 104 millemetres, all being + or deviations beyond the true middle.

In the normal position for the eyes at rest the visual axes are parallel and the point of convergence is infinitely distant. When the eyes are converged on a nearer object a voluntary effort is required, which is greater as the object is nearer. Any fatigue would therefore show itself in an over-estimation of the actual amount of effort put forth, and this would result in placing the object nearer to infinity or further away from the eyes in just the same way that when tired we would consider a quarter of a mile much greater than when fresh. In these particular experiments the near point and the far point were fixed; the fatigue would show itself in the change for the middle point in relation to the other two.

This change of the middle point can be attributed directly to fatigue. When the observer began in May he recorded that he was in excellent physical condi-

tion; outside work was, however, soon required, and the pressure was so great that he could go to the

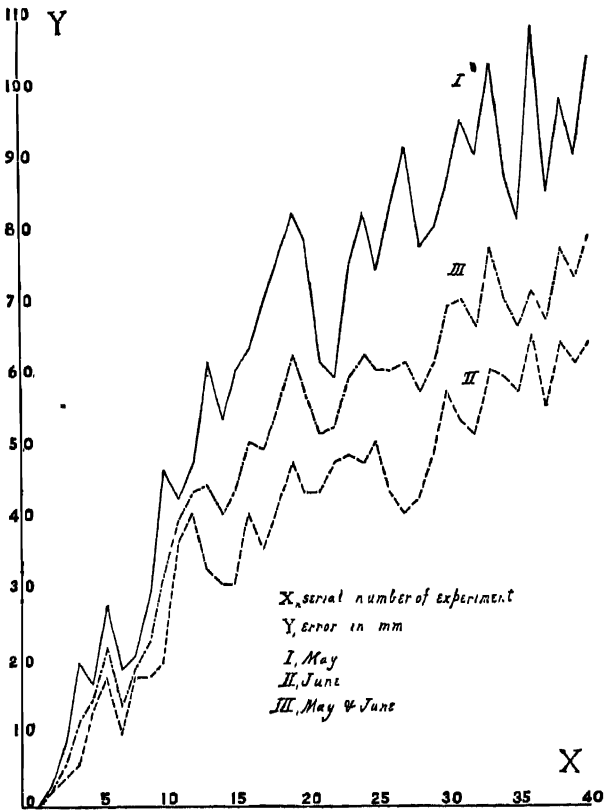


Fig. 59. CURVE OF FATIGUE FOR EYE MOVEMENTS (CONSTANT ERROR).

laboratory only after the most exhausting labour. During the last few days of the experimenting, a feeling of general physical depression was experienced

even at the beginning of the experiment. Further experimenting was postponed till June.

The June experiments were begun at the end of a month's work of the most exhausting character. The outside work had now narrowed to merely an employment which was not at all fatiguing, but which prevented any rest. The wear of the previous severe strain could not be easily repaired. There was very little change in the physical condition, and the record shows very slight variation. During July the work was steady, but there was little improvement in health, and the records showed steadily increasing fatigue.

The second question for consideration is the effect of the special fatigue in each experiment of the set of forty. In the previous paragraphs only the first experiment of the set was considered. With this first experiment taken as a standard, any deviation of the succeeding results from the same figure can be attributed to fatigue resulting from the previous experiments of the same set. Accordingly for each set the difference between each experiment and the first experiment was computed; thus, + 3 for the second experiment would indicate that the middle point was placed 3 mm. further from the eyes than in the first experiment. The result of the first experiment was thus used as a zero-point from which to reckon the effect of fatigue in the following experiments.

The average divergence of the second result from the first was + 3 for the eight sets in May, that of the third was + 9, &c. The results show an increase from the very beginning (Fig. 59).

The mean variation is, as we have seen, a valuable index of mental processes when all apparatus errors are, as here, practically eliminated. The mean variations of the results around each of the averages just given for

the first observer are shown in Fig. 60. It is evident that fatigue constantly decreases the regularity of executing the efforts.

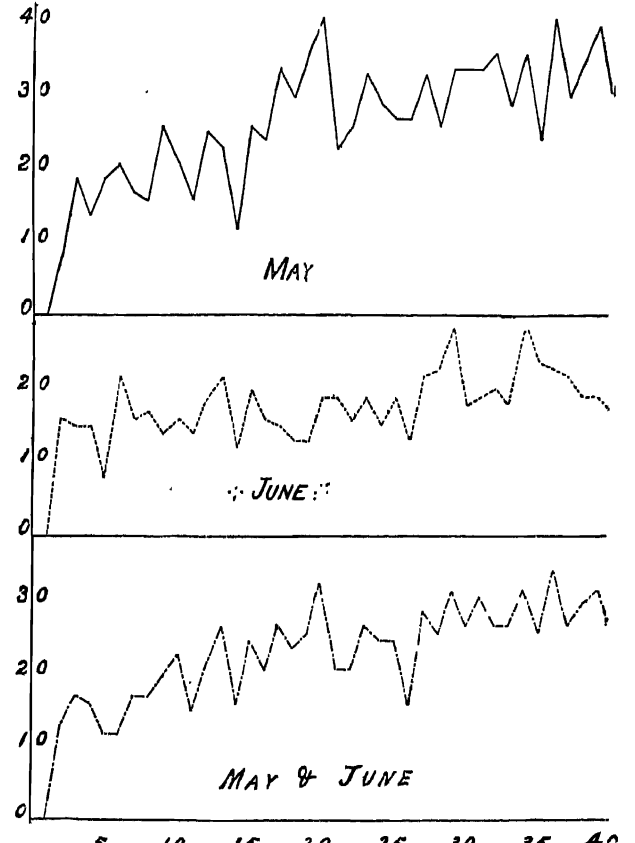


Fig. 60. CURVE OF FATIGUE FOR EYE MOVEMENTS (MEAN VARIATION).

The next point investigated was the effect of fatigue when the experiments were made with a single eye.

The left eye was placed directly in the line of the beads; the only work done by that eye was in accommodating the lens to different distances. The other eye, however, which did not see anything, was, as is well

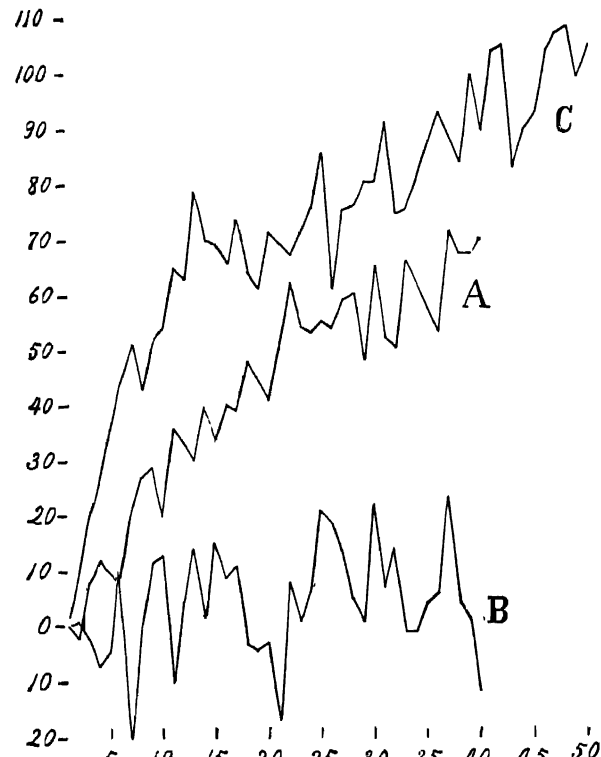


Fig. 61. CURVE OF FATIGUE WITH ONE EYE CLOSED.

known, converged and accommodated on each occasion for a point somewhat beyond the actual distance of the bead. There was thus a fatigue of accommodation in both eyes and of convergence in one; we would

naturally expect the effect to be similar but somewhat less than in the experiments with both eyes. The results for three different persons are shown in Fig. 61.

To eliminate the accommodation and convergence of the closed eye while retaining nearly all the accommodation of the seeing eye, an observer (C) was found who had lost his right eye. The results shown in the figure conclusively prove that fatigue of accommodation altered the estimate of depth. B showed no fatigue.

As fatigue enters into a voluntary act, does it increase the time necessary for executing that act? This question has been answered by Moore [1] for the act of accommodating the eye to different distances in an experiment similar to that just reported. The apparatus used was that invented by Seashore for the study of accommodation-time.[2] This apparatus consisted of a pneumatic camera-shutter to which electrical connections were added. There were two arrangements of the slide and the electric connections: (1) to drop the slide and expose the nearer point; (2) to raise the slide and expose further point. In the first arrangement one end of the electric wire was connected with the metallic body of the shutter. The other end was fastened to a binding post which was connected (1) with a wire spring which made contact with a projecting spring-arm on the slide at the moment the further point was cut off from view and the nearer point exposed, and (2) with a metal plate on which the projecting arm rested and made permanent contact when the slide came down. Both the contact-point and the metal plate were isolated from the metallic body of the shutter. In the second arrangement the slide was made to fly up and stop

[1] Moore, as before.

[2] Seashore, *On Monocular Accommodation-time*, "Stud. Yale Psych. Lab.," 1893, i. 56.

against a special catch. When the slide flew up, its projecting arm struck the special contact spring at the moment the nearer point was removed from view and the further point was exposed. The special catch against which the slide finally rested at the top, and with which it made permanent contact, was connected with the same binding post as the metal plate in the other arrangement. The current went through a closed-circuit reaction-key by means of which it could be interrupted. The current was made for an instant when the slide arm struck the special contact spring, permanently made by the slide arm resting on the metal plate or the special catch, and again interrupted by the reaction-key. The time required was that indicated between the first closing of the current and the breaking by the reaction-key. From the far object to the near object the distance was 825 cm., while from the near object to the eye it was 23 cm. The far object was a large capital E with a height of 25 mm., and the near object was a small capital E with a height of 7 mm., fastened on the slide of the camera-shutter. The far point remained stationary, while the nearer was presented or removed by a sudden movement of the shutter. A brass tube extended from the eye to the shutter. When the slide was up, the eye could see nothing except the large letter on the white background. The slide was raised and the observer focused on the far object. The experimenter touched a key which was electrically connected with a sounder in the recording-room. The recorder pushed down a key which closed the primary circuit. The experimenter now snapped the slide; this made a dot on the smoked paper and then closed the circuit. As soon as the observer saw the small E clearly and distinctly, he broke the circuit by the reaction-key, thus making

another dot on the smoked paper. When the recorder saw the two sparks, he raised the key and kept out all further sparks until the fifth repetition of the experiment in quick succession. A record was thus made of the time of every fifth experiment. The wave-lengths between the two dots could easily be counted and the time of accommodation obtained. This time included the subject's regular reaction-time.

The results for three different sets of experiments, namely, Seashore, Moore 1893, and Moore 1894, are

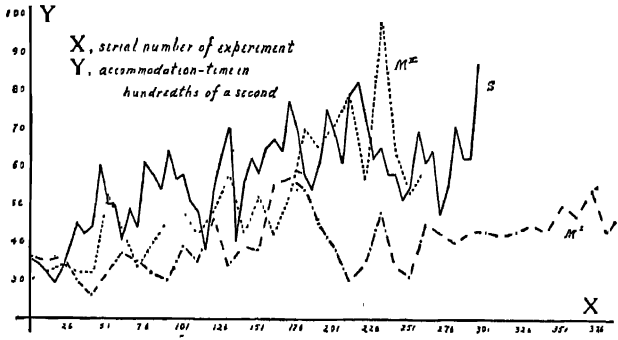


Fig. 62. INFLUENCE OF FATIGUE ON ACCOMMODATION-TIME.

shown in Fig. 62. It is conclusively proven that as fatigue enters in the time is persistently lengthened.

In Chapter VII. it was mentioned briefly that fatigue caused a change in the maximum rate of repeating a movement of the finger. A special investigation has been made[1] with very careful methods.

The subject was to begin when warned by a click from the sounder at his side and to continue to tap as rapidly as possible until his finger could move no more, or until told to stop. The finger was never completely

[1] Moore, *Studies of Fatigue*, "Stud. Yale Psych. Lab.," 1895, iii. 92.

fatigued because sufficient records could not be obtained upon the drum. "I once made 480 taps, but am quite sure that I could not have made 100 taps more at the close of any record, for it was always with great difficulty that the last 50 or 75 were made."

"The usual experience was as follows: Pain was first felt in the muscles of the forearm, then in the upper arm. Suddenly the blood rushed to the face and head, and the temples were filled with sharp pains. The whole right side (the work was done with the index finger of the right hand) seemed to partake of the trouble. After the experiment was completed, the arm seemed paralysed. I could not even handle the record-sheet with safety. The arm, especially the forearm, and often the shoulder would feel painful for a half hour afterward. Restoration was not complete in less than two hours and a half."

A specimen record was given in Fig. 30. The fluctuating nature of the tapping is made evident; quite noteworthy are the extremely long taps that occur with increasing frequency toward the end. In performing such experiments the subject when fatigued often feels for an instant a complete paralysis of the will. It is not a paralysis of the muscles with the intention to move still retained; on the contrary, the person feels absolutely lost and unable to will or desire anything for an instant.

In these experiments fatigue is generally shown by the time the 70th or 80th tap is made. A professional cornet player, who had not played for a year, showed no fatigue before 150 taps had been made. A woman who played on the piano, showed no fatigue until 200 taps were made. She affirmed that she felt no fatigue at all, but the incipient fatigue had left its traces in the records beyond the 200th tap.

In conclusion, it is perhaps desirable to point out the views expressed in this chapter that differ from the usual ones. "Fatigue" is defined as the decrease in the capacity for work; fatigue in this sense may or may not have definite relations to the peculiar sensation known as the "feeling of fatigue." Fatigue in voluntary action is at least partly fatigue of the will, as is made plainly evident not only by Lombard's experiments but also particularly by the inability to relax the muscles in the experiments described on p. 229. The major fluctuations, amounting to complete loss of power at what I have called "points of paralysis" but followed by immediate recovery, as first noticed by Lombard, I regard as specially characteristic of fatigue of the will. Finally, the chief "fatigue-functions" (if the term may be permitted) find their numerical expressions in the constant, or progressive, error (change in intensity, extent or time of the exertion) and in the mean variation, or mean error (change in regularity).

CHAPTER XVII.

PASSIVE AND ACTIVE MOVEMENT.

In experiments on passive movements Goldscheider [1] used an apparatus, of which the fundamental principles can be illustrated by its arrangement in studying the movement of the first interphalangeal joint of the index finger. The first phalanx of the finger was fixed on a plaster cast, and a thick close rubber shell was pushed over the last two phalanges. This shell hung from a broad band passing over an aluminum roller, 10 cm. in diameter, so that the line of the finger was perpendicular to the plane of the roller. Another band, hanging down from the roller on the other side, carried a small cork pan. By means of small weights, which could be put on the cork pan or on the finger-shell, the two sides of the apparatus could be balanced. A plate of lead on the cork pan maintained the last two phalanges in position without muscular tension. Small additional weights were laid on this lead plate and then removed, whereby passive movements, upward or downward, of the finger were produced. A fine lever was arranged, to write on the smoked drum. These records on the drum were carefully scaled in degrees of movement around the joint moved. This apparatus could thus

[1] Goldscheider, *Untersuchungen über den Muskelsinn*, "Archiv f. Physiol." (Du Bois-Reymond), 1889, 372.

produce movements of different velocities and distances as desired.

For movements with other joints, where the apparatus just described was not applicable, a hydrostatic method was employed. In a lower room a pressure pump forced water outward or inward, through a leaden pipe, connected to a second pump-cylinder in the experiment room, whereby the piston of the latter was moved upward or downward. The segment of the limb, *e.g.*, the forearm that was to be moved, rested in a support on the end of the piston-rod of this second pump.

In this manner, passive movements, *i.e.*, movements without any intention on the part of the subject, were produced with various degrees of distance and velocity. With extremely small movements nothing whatever was perceived. With somewhat greater movements, a peculiar, indefinite sensation was felt; this sensation was localised in the joint, but was not felt as a movement. With still greater movements, a sensation of movement appeared, in which, however, the direction of the movement was not apparent. Finally, when the movement had reached a certain size it was felt as a just perceptible passive movement.

The following table[1] gives the threshold of movement, *i.e.*, the just perceptible movement, around the various joints, for the greatest velocity obtainable without jarring. The figures indicate degrees.

2nd interphalangeal (between nail segment and middle segment of the finger) ...	1.03 to 1.26
1st interphalangeal (between middle and basal segments)	0.72 „ 1.05

[1] Goldscheider in "Zeitschrift f. Psych. u. Physiol. d. Sinn.," 1890 i. 146.

Metacarpo-phalangeal (between basal segment and hand)	0.34 to 0.43
Wrist	0 26 „ 0.42
Elbow	0.40 „ 0.61
Shoulder	0.22 „ 0 42
Hip...	0.50 „ 0.79
Knee	0 50 „ 0.70
Foot	1.15 „ 1.30

With other velocities the rule was found to hold that for those joints where the extent of the just noticeable movement was smallest, the necessary velocity was also the smallest. Finally, there appeared a special relation between the extent of the movement and its velocity, namely, that increased extent could make up for diminished velocity.

Special attention was paid to the question whether the threshold of movement for a given joint depended upon the position from which the movement was started; a noticeable dependence could not be found.

It was, moreover, conclusively proved that these sensations of movement do not depend upon sensations of pressure from the skin. They are connected physiologically with the membranes of the joints.

The question arises concerning the possibility that what has been called a sensation of movement may be derived from sensations of position.[1] These sensations of position are made up of pressure sensations from the skin, resistance sensations from the joints, heaviness sensations from the sinews, and possibly other sensations from other parts of the body, *e.g.*, muscles, periosteum, &c. These sensations enter into different combinations with each position of the part moved. It is unquestionable that in any movement executed, these

[1] Müller and Schumann, *Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte*, "Archiv f. d. ges. Physiologie" (Pflüger), 1889, xlv. 71.

sensations play more or less important parts. Goldscheider has proved, however, that special sensations of movement exist in addition to them. By faradisation of the finger the knowledge of position could be completely removed and yet the sensitiveness to movement was still present and was not necessarily diminished. Again, the existence of a movement could be perceived when the movement was not great enough to give a knowledge of its direction. The existence of a special sensation of movement can be regarded as conclusively proven.

The size of the just perceptible passive movement has been found to be subject to several remarkable influences. In the first place, children are much more sensitive than adults, whereby it is to be remembered that the movement is measured as an angle. Such a result seems quite opposed to the results of experiments on the blind.[1] Blind persons who are practised in feeling are much more sensitive than other persons. The cause of this increase in sensitiveness lies in sharpening of the attention, and in practice in the interpretation of sensations whereby scarcely perceptible sensations can be made use of.

Up to this point the movements have been passive; in "active" movements other elements are added. There are present the intention to perform the movement and the volition which causes its performance. Goldscheider measured the just perceptible active movements with the same apparatus as for passive movements. The experiments showed that the least perceptible movement was practically the same for both active and passive movements. Faradisation of the joint reduced the ability to say whether the

[1] Hocheisen, *Ueber den Muskelsinn bei Blinden*, "Zt. f. Psych. u. Phys. d. Sinn.," 1893, v. 239.

intended movement had actually been executed or not. For example, for the first interphalangeal joint the just perceptible passive movement was 0.72° to 1.05°; for active movement it was 0.76° to 0.85°;[1] with a current through the joint it was, for passive movements 3.66°, and for active movements 3.09°, the reduction of sensitiveness being about the same in both cases. The knowledge of the presence of movement seems, therefore, to depend practically entirely on the sensations of movement and not on the intention or the volition.

This fact is brought out by Goldscheider's experiments[2] on imperceptible active movements. The end phalanx of the index finger was placed on a rubber capsule such that the slightest impression caused a recording pointer to make a mark on the smoked drum. Sensations of touch were deadened by a thick rubber cap on the finger.

As long as the subject could say definitely that he had not moved his finger the pointer remained still. Although Goldscheider does not say so expressly, this is presumably quite independent of any mere memories of the movement. The person then willed a slight movement of the finger. The condition was found in which he believed that he had moved it, but was not sure whether the movement was real or only imaginary. At every such experiment, however, there was a slight excursion of the recording pointer.

The execution of a movement, whether passive or active, is made known to us by means of sensations from the part moved. If they are present without any intention to move, the movements are considered passive. If they agree with the movements intended,

[1] Goldscheider, *Unters. ü. d. Muskelsinn.*, "Arch. f. Physiol." (Du Bois-Reymond), 1889. Suppl. Bd., 207.

[2] Goldscheider, as before, p. 211.

we consider them to be active movements. If they are unclear, we are doubtful if our intention has been executed.

Active movements vary between two extremes, voluntary active movements, and involuntary active movements.

The complete process that occurs during a voluntary active movement contains, in the first place, an idea of the movement to be performed. Then follows a process of willing to execute this idea. Finally, there result certain sensations which tell us that the movement has actually been executed. It is necessary that these latter sensations agree with the original idea of the movement, if the movement is to appear voluntary. If the two do not agree, as in the case of certain muscular disturbances whereby the movement occurs in a different direction from the intended one, then the person feels as if he were subjected to a passive movement whereby his own intention was overcome.

In what is called an involuntary movement the preliminary idea of the movement to be performed is lacking; the person acts without intending to do so. Even a fixed determination to remain inactive would hardly prevent the ordinary man from starting at an unexpected shrill sound.

The unperceived involuntary movements are at the bottom of the phenomena of "table tipping," and "thought transference." Perhaps I ought not to mention such subjects without repeating Faraday's apology for his experimental investigation.[1] "I am a little ashamed of it, for I think in the present age, and in this part of the world, it ought not to be required."

[1] Faraday, *Experimental Investigation of Table-Moving*, "Athenæum," 1853, July 2; also in "Experimental Researches in Chemistry and Physics," 390, London, 1859.

But there is a particular reason why the psychologist should have his attention called to them. Faraday wished merely to show that the phenomenon of table tipping was not a physical one, but a psychological one; this, however, brings the matter home to the psychologist since it is part of his business to investigate unperceived involuntary actions.

Faraday's investigation was, as usual, a model of experimental methods and deductions.[1]

"The effect produced by table-turners has been referred to electricity, to magnetism, to attraction, to some unknown or hitherto unrecognised physical power able to affect inanimate bodies—to the revolution of the earth, and even to diabolical or supernatural agency. The natural philosopher can investigate all these supposed causes but the last; that must, to him, be too much connected with credulity or superstition to require any attention on his part.

"Believing that the first cause assigned—namely, a quasi involuntary muscular action (for the effect is with many subject to the wish or will)—was the true cause; the first point was to prevent the mind of the turner having an undue influence over the effects produced in relation to the nature of the substances employed. A bundle of plates, consisting of sand-paper, millboard, glue, glass, plastic clay, tinfoil, cardboard, gutta-percha, vulcanised caoutchouc, wood, and resinous cement, was therefore made up and tied together, and being placed on a table, under the hand of a turner, did not prevent the transmission of the power; the table turned or moved exactly as if the bundle had been away, to the full satisfaction of all present. The experiment was repeated with various substances and persons, and at various times, with constant success; and henceforth no objection could be taken to the use of these substances in the construction of apparatus. The next point was to determine the place and source of motion, *i.e.*, whether the table moved the hand, or the hand moved the table; and for this purpose indicators were constructed. One of these consisted of a light lever, having its fulcrum on the table, its short arm attached to a pin fixed on a cardboard, which could slip on the surface of the table, and its long

[1] Faraday, *On Table Turning*, "Times," 1853, June 30; also in "Experimental Researches in Chemistry and Physics," 382, London, 1859.

arm projecting as an index of motion. It is evident that if the experimenter willed the table to move towards the left, and it did so move before the hands, placed at the time on the cardboard, then the index would move to the left also, the fulcrum going with the table. If the hands involuntarily moved towards the left without the table, the index would go towards the right; and, if neither table nor hands moved, the index would itself remain immovable. The result was, that when the parties saw the index it remained very steady; when it was hidden from them, or they looked away from it, it wavered about, though they believed that they always pressed directly downwards, and when the table did not move, there was still a resultant of hand force in the direction in which it was wished the table should move, which, however, was exercised quite unwittingly by the party operating. This resultant it is which, in the course of the waiting time, while the fingers and hands become stiff, numb, and insensible, by continued pressure, grows up to an amount sufficient to move the table or the substances pressed upon. But the most valuable effect of this test-apparatus (which was afterwards made more perfect and independent of the table) is the corrective power it possesses over the mind of the table-turner. As soon as the index is placed before the most earnest and they perceive—as in my presence they have always done—that it tells truly whether they are pressing downwards only or obliquely, then all effects of table-turning cease, even though the parties persevere, earnestly desiring motion, till they become weary and worn out. No prompting or checking of the hands is needed—the power is gone; and this only because the parties are made conscious of what they are really doing mechanically, and so are unable unwittingly to deceive themselves."

These involuntary active movements are clues that reveal a person's thoughts to the muscle reader. In most such cases, the movements are so faint that the subject does not know of their existence, whereas the muscle reader feels them. A characteristic case is Cumberland's first experiment.[1]

"I took my host by the hand . . . and led him from the breakfast-room; not quickly as I do now, but slowly and lingeringly.

[1] Cumberland, *A Thought Reader's Experiences*, "Nineteenth Century," 1886, xx. 867.

We entered the study, and I immediately felt that I was in the correct locality. A moment more and I placed my hand upon an object, which, according to the impression I then received, I believed to be my subject's selection. I was quite right."

A still more striking illustration of involuntary active movements is found in another case :—

"We then resumed connection with the hands, and in another moment I found myself flying across the room. In my experiments I always take the lead ; but in this case my 'subject' took it."

Cumberland afterwards tells the subject :—

"I felt you were so intent on 'willing' me to go to the spot, that in the very intensity of desire, you unconsciously dragged me the whole of the way ; I did nothing but remain quite passive, until I came to the table where the toy was, and common sense told me to lift up the tambourine and take it out."

Cumberland's statement of the source from which he derives his knowledge, is as follows :—

"In my case, 'thought-reading' is an exalted perception of touch. Given contact with an honest, thoughtful man, I can ascertain the locality he is thinking of, the object he has decided upon, the course he wishes to pursue, or the number he desires me to decipher almost as confidently as though I had received verbal communication from him."

The complex character of the involuntary twitchings is illustrated by the following case. In describing an experiment before the Khedive, Cumberland relates :

"Paper and pencils were brought and a sheet of the former was gummed upon one of the gilded doors. The Khedive thereupon thought of a word, and, without any sort of hesitation, I wrote on the paper the word Abbas (the name of his son) in *Arabic characters.* I did not know at the time a single letter of the Arabic alphabet; and . . . the experiment was entirely impromptu."

The important point in these matters lies in the suggestion for systematic quantitative study of the

manner in which the amount of energy expended in these unconscious movements depends on the various mental conditions. This can probably be done with dynamometers of various strengths (Figs. 4 and 24).

Faraday's apparatus gives the suggestion for a complete apparatus that can be arranged in the laboratory. The subject's arm is supported on a plate hanging from a pulley overhead; the amount of muscular tension desired is regulated by a weight at the other end. The force of the movement in any direction can be measured by arranging a dynamometer so that the plate pushes against it. To measure the complete exertion in all directions, three dynamometers are to be attached to the plate, so as to act in the three cardinal directions of space; the records can be made on the drum by three piston-recorders.

A study of unconscious movements of the arm has been made by Jastrow,[1] who found that a person tends in general to move toward an object to which attention is being paid. Delabarre's suspended planchette apparently renders it possible to enter upon a further scientific exploration of unconscious movements. The subject's hand or arm is rested on a small board suspended by cords from a high ceiling. A recording point attached to the board leaves a record of every movement. Delabarre finds that looking or listening with close attention to an object induces in some subjects an immediate and decided movement of the arm toward the object; in others, a similar movement after more or less considerable delay; in others still, an apparent lack of effect, or even a movement in the opposite direction. Thinking of an object in some definite position is attended by similar movements. The result

[1] Jastrow, *A Study of Involuntary Movements*, "Am. Jour. Psych.," 1892, iv. 398.

depends to a very great degree upon the success with which the subject can withdraw his attention from his arm and its movements, and concentrate it entirely upon the object.

The suspended planchette has been used in experiments by Delabarre on cultivated automatic movements in normal persons, in imitation of Binet's experiments on double consciousness. The conclusion was reached that, provided the attention can be completely absorbed elsewhere, it is possible to cultivate in probably all persons automatic movements of greater or less complexity, which continue indefinitely, or start, stop, or change their character, in accordance with slight, unconsciously received suggestions communicated to the arm. Two subjects, both of them entirely normal in temperament and health, exhibited similar phenomena with the suspended planchette as a support for the arm experimented with. Attention was distracted by reading or by conversation, and the experimenter then communicated to the subject's arm movements of various types. After continuing to impress these movements for some time, the experimenter endeavoured to discontinue his own pushing movements without attracting the subject's attention, in order to see if the arm would continue its movements automatically. This did not occur at first, or occurred only slightly and at intervals. But after several sittings marked success was attained. The forms of movement tested were the straight-back and forward, the elliptical, the circular, and movement in form of the figure 8. Very slight touches were found sufficient to start, stop, or change the movement. The hand would also itself sometimes initiate movements, and continue them for several minutes, even when the experimenter was at some distance from the subject—the latter having by this time gained sufficient skill

in withdrawing attention from his arm to make him unconscious of its movements, thus favouring its execution of the automatisms it had learned. The degree of distraction of attention of course varied from time to time, and always with marked effect upon the automatic movements. For instance, whenever a page was turned in reading, the experimenter could from the first detect an increased resistance and hesitation in the arm. Animated conversation was found the most successful means of distraction, and during its progress the movements would proceed quickly or slowly in apparent variation with the interest and animation of the subject.

These results add confirmation to Binet's conclusion, that it is possible to cultivate in normal persons motor automatisms, of a simple nature at least, analogous to the phenomena presented in cases of so-called "double-consciousness." [1]

A most interesting case of these unconscious movements is found in involuntary whispering. In the experiments of Hansen and Lehmann referred to in Chap. IV., the subjects noticed a marked tendency toward action of the muscles of speech whenever a number was thought of for a while. This tendency was suppressed by a special effort. When no such effort was made, the one subject with his ear at the focus of the mirror could hear the involuntary sounds produced by the other one, in spite of the fact that the mouth was kept tightly closed and a watcher standing near by could detect no sounds or movements whatever. The mirrors made the sounds 14 times stronger for the subjects, so that the involuntary nasal whispering was sufficiently loud enough to give a large percentage of successful transferences of thought. For thought-transference, therefore, all that is required is to find a subject

[1] Binet, *Double Consciousness in Health*, "Mind," 1890, xv. 46.

who has an abnormally sharp ear and, for your part, to think very intently on the word you wish transferred. It is not necessary that there shall be any intentional communication; if the investigators are sufficiently untrained in scientific psychological experimenting, and are inclined to attribute results to occult powers rather than to their own incapacity, the proofs of thought-transference inevitably follow. A careful study of the mistakes in the transferred figures, *e.g.*, 10 for 18, 37 for 66, &c., proves that they arise from close similarities of the sounds in nasal whispering. One subject of a thought-transference experiment remarks that something seemed to tell him that the number was so and so. This something apparently never told him right, yet in the whisper-language the mistakes are those of very similar sounds. It is quite evident what this "something" was.

Thus ends the great mystery of thought-transference. The transference by contact proved to be the communication of involuntary, unconscious movements of the hand; the transference without contact proves to be the production of the involuntary, unconscious movements employed in nasal whispering.

CHAPTER XVIII.

RESISTANCE AND HEAVINESS.

In this chapter we shall consider two different sensations, resistance and heaviness, with their variations in intensity.

When we touch an object with a stick held in the hand, we notice a peculiar sensation which is referred to the extreme end of the stick. We apparently feel the resistance to the movement of the stick. It is this sensation that we are first to consider.

Before proceeding to further treatment, it is necessary first to prove that resistance is not simply a complex of pressure-sensations from the skin, and sensations from the muscles. This has been done by Goldscheider.[1]

In the first place, the skin sensations are not the essential factors. If they were, an increased pressure on the skin would disturb the sensation of resistance. Let the table be touched with a pencil held lightly in

[1] Goldscheider, *Untersuchungen über den Muskelsinn*, "Archiv f. Physiologie" (Du Bois-Reymond), 1889, Suppl. 165. The accounts of Goldscheider's work as presented in this chapter, are taken from the article just cited and from a summary, by Goldscheider himself, of this and another article in the "Zeitschrift f. Psych. u. Physiol. d. Sinnesorgane," 1889, i. 145; Goldscheider's own words are used as often as practicable.

the fingers; a resistance is felt. Now let the experiment be repeated with the pencil gripped as tightly as possible with the fingers; the pressure sensations may be so strong as to be painful, yet the sensation of resistance is apparently unchanged. If resistance consisted in slight additional pressure on the skin this result would be contrary to all our experience, according to which slight additions to strong sensations have less effect than to weak ones. Again, let a rubber air-cushion be placed on the end joint of a finger, and a string carrying a weight be placed around the cushion. Such a cushion reduces the sensation of pressure to an extremely small amount, yet when the weight begins to draw on the string as the hand is raised from beside it, the resistance is plainly felt. Finally, when by an electric current the sensitiveness of the end section of the finger is so far reduced that the skin cannot feel less than a strong impression from a blunt scissors point, even then a touch of the finger on a table is felt with very little loss of intensity. It is very clear, therefore, that the sensation of resistance may be independent of pressure.

This independence is emphasised in cases of anæsthesia of the skin such as occurs in tabes, as is illustrated by the following case. The skin of the patient's heel was so completely insensitive, that he did not feel the pinching of the skin or deep pricks with a needle. Yet he felt with the greatest sureness and regularity the lightest tapping of a finger in the direction of the axis of the limb.

The actual relation of pressure-sensations to sensations of resistance is brought out in the "resistance paradoxes." Let a heavy weight be held by a string from the fingers or from a stick held in the hand. Lower the weight rather rapidly till it rests on a cushion or a box

of sand. As it strikes, a vivid sensation of resistance to the movement is felt, somewhat as though the hand were suddenly supported by a rod. The illusion is most marked when the length of the string is unknown to the person holding it; this is readily brought about by having some one to prepare the weight and hand it to another person with eyes closed. This illusion is due to the sudden release of muscular strain, whereby the muscles themselves produce a pressure on the joints similar to one that would be produced by actually striking an object.

A similar sensation is produced by stretching a thin elastic band with the two hands, then relaxing it. As soon as the strain is completely relaxed a sudden blow is apparently felt on the finger ends.

This phenomenon has been used to investigate the relation between resistance-sensations and pressure-sensations.[1]

The apparatus consisted of a string passing over two pulleys, to avoid side-movements, and carrying a weight. The string was fastened to a band which passed around a rubber cuff filled with water. Different cuffs were used for finger, hand, forearm, and upper arm; they greatly reduced the sensation of pressure.

The weight was lowered with a velocity of about 6 cm. per second; it came to rest on a sand-bag. The weight used was an aluminum pan filled with shot. The weight was increased from an imperceptible value until it was first noticed; then it was decreased from a more than perceptible value till it ceased to be noticed.

The following are some typical results:

For movements around the shoulder joints; (*a*) string

[1] Goldscheider and Blecher, *Versuche über die Empfindung des Widerstandes*, "Archiv f. Physiol." (Du Bois-Reymond), 1893, 536.

on the end of the index finger, 8.1 g.; (*b*) on the second phalanx, 11.0 g.; (*c*) on the first phalanx, 15.1 g.; (*d*) on the hand, 25.3 g.; (*e*) on the forearm, 44.8 g. to 58.5 g.; (*f*) on the upper arm, 77 4 g.

For movements around the elbow; (*a*) on the end phalanx, 9.7 g.; (*b*) on the second phalanx, 11.4 g.; (*c*) on the first phalanx, 17.8 g.; (*d*) on the hand, 31.2 g.; (*e*) on the forearm, 71.9 g. These results were obtained by a movement of the forearm from a position vertically downward to a horizontal one; for other positions they differed somewhat.

For movements around the metacarpo-phalangeal joints; (*a*) on the end phalanx, 22.6 g.; (*b*) on the second phalanx, 66.3 g.

After the sensitiveness was determined under these conditions for the various joints, pressure sensations were allowed to enter. The band was placed directly on the skin, the rubber cuff being omitted. The result was unexpected; a sensation was always perceived with a weight of 10.6 g. to 12.8 g.; regardless of where the band was placed and of which joint was moved. In fact, two sensations were present; one of resistance which was localised where the weight struck, and one of pressure localised where the band touched the skin. This separation of the two sensations was lost with the smaller weights, with which the pressure-sensation was absorbed into the sensation of resistance.

Even when thus absorbed, the pressure-sensation influenced the sensation of resistance as was proven by the following experiments. In one set the band was placed tightly around the finger, in another it was left loose, and in a third the skin was strongly compressed by a metal covering; thus the pressure-sensations from the band were most delicate in the second set, somewhat overpowered in the first set, and

strongly overpowered in the third set. The results were as follows :—

Joint moved.	Tight band	Loose band	Compression of skin.
Metacarpo-phalangeal	60.0	47 3	over 300
Wrist	45.0	17 8	33.8
Elbow	10.0	13.8	20.5
Shoulder	9.4	13 4	14 5

In this table the two influences are apparent. Reading vertically downward we find increased sensitiveness as depending on the sensations of resistance from the various joints. Reading horizontally we see the influence of the pressure-sensations.

The pressure-sensations, therefore, under ordinary circumstances, enter into our experiences of resisting bodies to a certain degree, although they are quite distinct from the sensations of resistance themselves.

Goldscheider's investigations also plainly prove[1] that none of the other kinds of sensation, such as those derived from the muscles and the sinews, take any part in the feeling of resistance. They also prove that, on the physiological side, the concomitant processes are started in the joints.

Another one of our sensations is that of heaviness. When we lift a weight fastened to a string we notice a certain feeling which we describe by saying that the weight is heavy; what we actually feel is a more or less complicated sensation that we call heaviness.

Weights can be lifted by movements where only one joint is involved, or by movements where several are involved. The former class includes those by the end joints, or by other joints when the more extreme ones

[1] Goldscheider, *Untersuchungen über den Muskelsinn*, "Archiv f. Physiol." (Du Bois-Reymond), 1889, 167 to 171.

are eliminated by a stiff bar. The latter class involve more or less complicated relations of all the joints beyond the one at which the movement is made.

The sensations in the two cases are different. When the movement is so made that it starts at first free, but suddenly receives a weight, *e.g.*, by lifting a weight with a loose string, the effect with a single joint is merely that of a hindered movement. When this is done with several joints there is a feeling of resistance at the moment the weight is lifted. In this latter case heaviness and resistance are both present, in the former only heaviness.

Goldscheider's results prove that the sensation of heaviness is quite independent of the sensations of pressure and of resistance. Physiologically it is closely connected with the tension of the sinews. Psychologically it enters into frequent connection with sensations of movement to produce ideas of moving heavy objects, with sensations of resistance to produce ideas of inertia, and with sensations of pressure to produce ideas of supporting heavy objects.

Most of our judgments of heaviness are made with the aid of movements; we can best consider the particular experiments in a chapter on lifting weights.

CHAPTER XIX.

LIFTING WEIGHTS.

ALL movements require the expenditure of energy in transferring the object moved. The amount of work done depends on the mass of the object moved and the distance of the movement. In moving a portion of the body we move a larger or smaller mass of bone, muscle, &c. When moving an object the weight of this object is added to that of the portion of the body involved.

The simplest case for investigation of the work involved in moving objects is that found in lifting weights. This case is of peculiar interest from the fact that with Weber and Fechner it played an extremely important part in establishing the new psychology.

Weber's experiments[1] consisted in finding how much a weight must be decreased in order that the difference shall be just noticeable. He found that for a weight of 32 drachms the average difference for four persons was 3 drachms, and for a weight of 32 ounces it was not 3 drachms, but 3 ounces. In other words, the just perceptible difference for a lifted weight was not a constant amount, but was a constant fraction of the

[1] Weber, "Annotationes anatomicæ et physiologicæ," 81, Leipzig, 1851. Weber, *Tastsinn und Gemeingefühl*, "Wagner's Handwörterbuch der Physiol.," iii. (2) 559 ; also separate, 104.

weight lifted. This fact of proportionality of the just perceptible difference received from Fechner the name of Weber's law.[1]

Fechner began his experiments for the purpose of more accurately testing Weber's law. He soon found that the problem required a previous development of the method of experiment itself and the rules of computing its results. For several years Fechner regarded it as a kind of daily work to perform experiments for about an hour for this purpose. The results, as far as determining the just noticeable difference, are not of very great value, owing to the extremely complicated character of the judgments involved in comparing lifted weights. They furnished, however, for the first time, a carefully developed method for measuring judgments of this character. This method of right and wrong cases, as it was called, was worked out by Fechner[2] and Müller;[3] it maintains its place as a foundation-stone of the new psychology.

The method of right and wrong cases is, briefly stated, as follows: When the difference between the two weights is very small, the person lifting them will frequently judge the lighter one to be the heavier, and the reverse; the greater the difference between the weights, or the finer the sensitiveness of the person, the greater will be the proportion of correct judgments to wrong ones. The method of right and wrong cases consists in determining the amount of difference required to produce a given relation of right cases to the total number. The sensitiveness of the person lifting these weights is to be considered as inversely propor-

[1] Fechner, "Elemente der Psychophysik," 134, 2 ed., Leipzig 1889. Müller, "Zur Grundlegung der Psychophysik," Berlin, 1878. This contains a full account with discussion.

[2] Fechner, as before, 69, &c. [3] Müller, as before.

tional to the difference thus found. Moreover, if Weber's law be true, this difference should be a constant fraction of the weight lifted.

Take the case of two lifted weights, R_1 and R_2, with a small difference, $D = R_1 - R_2$. The subject will on each occasion decide, "First greater than second," "Equal or doubtful," or "First less than second;" that is, he passes one of the three judgments $D > 0$, $D = 0$, $D < 0$. Let r, g, and f, be the number of judgments thus passed in a series of n experiments; how shall we state the results? If there were only two judgments, the case would be simple enough; the existence of three has led to an immense amount of discussion and to all kinds of opinions.

Some people have thought it sufficient to state the percentage of right answers alone or of wrong answers alone; this procedure can hardly be justified. Fechner and most others have divided the g cases equally between the right and the wrong, and have established a table whereby we can find, for any desired value of D, the number of right answers (including half of the g cases) for experiments under the same circumstances, provided we know the number for any one value of D (see Appendix VIII).

The troublesome question of distributing the g cases between the r cases and the f cases we can pass over entirely. The whole question has arisen solely from the fact that of two possible methods of computing results, the least appropriate one was chosen. It remained for an astronomer to put an end to the apparently endless discussion by showing that the standard methods for adjusting measurements, as employed in astronomy, physics, &c., were able to cover this case also.[1] The

[1] Bruns, *Ueber die Ausgleichung statistischer Zählungen in der Psychophysik*, "Phil. Stud.," 1893, ix. 1.

formulas for computing the results are of interest only to the specialist, but the general idea of the method can be illustrated as follows.

Suppose the true difference OD to be laid off in the line AB from the zero-point o. The difference, as observed by the subject, varies continually. There is a region Z_u to Z_o, in which he judges $D = o$; another region, Z_o to $+\infty$, in which he judges $D > o$; and a third, Z_u to $-\infty$, in which he judges $D < o$. Bruns's method determines the values for Z_o and Z_u, or the limits of the region of uncertainty, Z_u to Z_o. This index of uncertainty is what is desired.

Fechner's results[1] showed an approximately constant fraction only for moderately heavy weights. Later

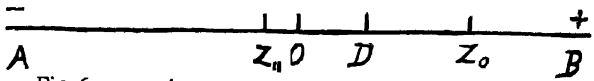


Fig. 63. BRUNS'S METHOD FOR RIGHT AND WRONG CASES.

experiments by Merkel,[2] whereby the just perceptibly different weight was selected from a series, showed that the fraction changed greatly for both very heavy and very light weights, but was fairly constant for medium weights.

We can thus accept Weber's law as valid, with fair approximation, for moderate weights.

By making two noteworthy assumptions Fechner transformed Weber's law into a general form.

The first assumption is that the just perceptible difference is a constant psychological quantity; *e.g.*, if the just perceptible difference is found for a weight of 10 grammes, and the just perceptible difference is found

[1] Fechner, as before.

[2] Merkel, *Die Abhängigkeit zw. Reiz u. Empfindung*, "Phil. Stud.," 1889, v. 253.

for one of 10 kilos., the two differences mean the same thing to us mentally. If we denote the just perceptible difference in sensation by ΔE, Weber's law becomes, on this assumption, $\Delta E = C \frac{\Delta R}{R}$, where C is a constant quantity. This holds good for all values of R.

The second assumption is that what is true for the finite differences, ΔE and ΔR, is also true for the infinitely small differences, dE and dR. Then it

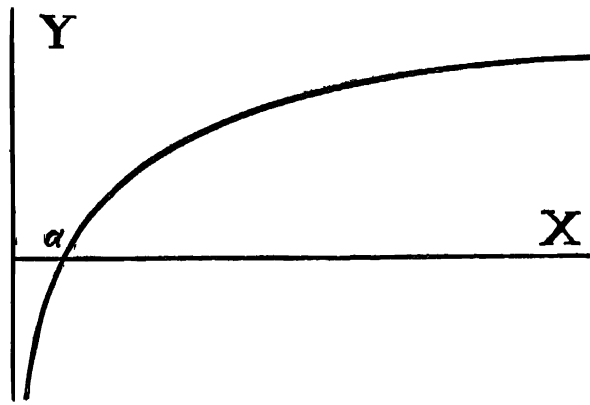


Fig. 64. FECHNER'S LAW OF THE RELATION BETWEEN STIMULUS AND SENSATION.

follows that $dE = C \frac{dR}{R}$, which is Fechner's fundamental formula.

From Fechner's formula it follows by integration that $E = C \log R + A$. At the threshold of sensation we have $E = 0$, with a value a for R; this gives— $A = -C \log a$; and $E = C (\log R - \log a)$. Using $a = 1$ as the unit by which to measure R, we have Fechner's law $E = C \log R$. This law is represented by the curve in Fig. 64.

The intensities of the stimulus are laid off on the axis of X, and those of the sensation in the axis of Y. At *a* is the point where the stimulus first becomes noticeable. The intensity of the sensation thus varies as the logarithm of the stimulus.

In investigating our judgments of heaviness we find that a judgment is greatly influenced by preceding judgments.

Fechner noticed that after a long series of experiments with heavy weights, a lighter weight appeared unusually light.[1] The experiments were extended by Müller and Schumann, who proved that as the difference between the weights lifted was made greater, the change in the estimates became greater, and that the influence of such differences diminished with the lapse of time.[2]

The expected heaviness of a lifted weight depends not only on our acquaintance with it by actual lifting, but also on our general acquaintance with the looks and feeling of light and heavy bodies. When a weight looks large we expect it to be heavy. We consequently prepare for strong sensations when we lift it. If we do not realise these sensations, we believe it to be lighter than it really is. The reverse likewise holds true.

The amount of this illusion was measured by Gilbert.[3] His object was to determine its relation to age; his apparatus, therefore, provided for a single case, which was kept constant throughout. It consisted, in the first place, of a series of fourteen round blocks painted black;

[1] Fechner, *Ueber die Contrastempfindung*, "Ber. d. k. sächs. Ges. d. Wiss., math. phys. Cl.," 1860, xii. 76.

[2] Müller and Schumann, *Ueber die psycholog. Grundlagen der Vergleichung gehobener Gewichte*, "Arch. f. d. ges. Physiol." (Pflüger), 1889, xlv. 37.

[3] Gilbert, *Researches on the Mental and Physical Development of School Children*, "Stud. Yale Psych. Lab.," 1894, ii. 43, 59.

in appearance they were all just alike, but in weight they were different. There were two other blocks, one much larger, and one much smaller than the series. The person was required to pick out that block of the series which felt equally heavy with the large block, and also that block which felt equally heavy with the small one. The true weights were, of course, unknown to him. Both the large block and the small block weighed 55 grammes, the blocks of the series ranged from 15 grammes to 80 grammes. The difference between the weight picked out for the larger one, *e.g.*, 20 grammes, and that for the smaller one, *e.g.*, 70 grammes, would give the effect of the difference in size between the two blocks. The difference in weight in this example would be 50 grammes, which would be the result of the difference of six centimetres in the diameter of the blocks.

The effect of the suggestion depends upon the age. In New Haven about 100 children of each age from 6 to 17 were tested by Gilbert. The average effect of the suggestion was as follows: 6 years, 42 grammes; 7 years, 45 grammes; 8 years, 48 grammes; 9 years, 50 grammes; 10 years, 44 grammes; 11 and 12 years, 40 grammes; 13 years, 38 grammes; 14 to 16 years, 35 grammes; 17 years, 27 grammes. For all ages the average was above 25 grammes. The suggestibility slowly increases from 6 years to 9 years; after 9 years it steadily decreases as the children grow older. The results, when separately calculated for boys and girls, show that at all ages the girls are more susceptible to suggestion than the boys, with the exception of the age 9, where both are alike extremely susceptible.

These are the average results for large numbers of children. Many young people, however, were so sus-

ceptible that the set of middle-sized blocks did not range far enough to suit them. At the age of 7 years 37 per cent. of the children declared that the large block was lighter than the lightest block, and that the small block was heavier than the heaviest. The actual difference between them was 65 grammes; thus the effect of suggestion was more than the weight of the suggesting blocks.

The idea involved in Gilbert's experiment has been made a subject of an extended investigation by Seashore.[1]

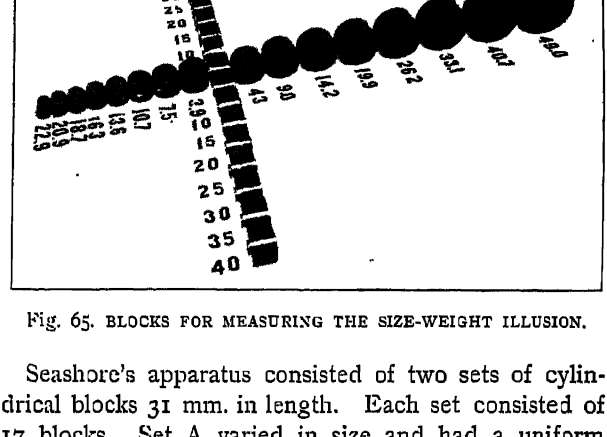


Fig. 65. BLOCKS FOR MEASURING THE SIZE-WEIGHT ILLUSION.

Seashore's apparatus consisted of two sets of cylindrical blocks 31 mm. in length. Each set consisted of 17 blocks. Set A varied in size and had a uniform weight, while Set B varied in weight and had a uniform size. The blocks in Set A varied in diameter according to a series in which the regular increment is one-tenth. Those in Set B were arranged in a

[1] Seashore, *Measurements of Illusions and Hallucinations in Normal Life*, "Stud. Yale Psych. Lab.," 1895, iii. 1.

series according to weight with a constant difference of 5 g.

In the following account the blocks will be distinguished by the names A and B with their respective numbers in the series.

The blocks of Set A were of a constant weight, 80 g, and of diameters in millimetres as follows, beginning with the smallest: 20.0, 22.0, 24.2, 26.6, 29.3, 32.2, 35.4, 39.0, 42.9, 47.2, 51.9, 57.1, 62.8, 69.1, 76.0, 83.6, 91.9.

The blocks of Set B were of a constant diameter, 42.9 mm., and of weights in grammes as follows, beginning with the lightest: 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90, 95, 100, 105, 110, 115, 120.

It is to be observed that the uniform weight for Set A is the same as the weight of B (9), the middle block in Set B; and the uniform size in Set B is the size of A (9), the middle block in Set A.

The observer placed himself by the table, on which the blocks were arranged in order, in such a position that by moving back and forth he could lift any block from its place in Set B and still retain approximately the same angle of the arm and hand. He was requested to select for each block in Set A a corresponding one in Set B, by taking one at a time from A and placing it by the side of successive blocks in B with which he wished to compare it, lifting one at a time until he found the one in B which he thought had the same weight as the one from A.

The results are given in the following table:—

ILLUSION OF WEIGHT WHEN THE BLOCKS ARE SEEN DIRECTLY.

A, size of the block in Set A (having a weight of 80 g.).

B, weight of the block in Set B (having a diameter of 42.9 mm.) chosen as equal in weight to the block of Set A.

C, number of millimetres by which the diameter in Set A differed from that in Set B.

D, grammes by which the estimated weight of the block in Set A differed from its true weight; average of a total of twenty-five experiments on fifteen persons.

MV, mean variation; to obtain the mean variation for the series each result is to be divided by five.

A	*B*	*C*	*D*	*MV*
20.0	110.2	—22.9	+30.2	7.5
22.0	103.8	—20.9	+23.8	7.0
24.2	98.2	—18.7	+18.2	5.0
26.6	94.4	—16.3	+14.4	6.5
29.3	94.0	—13.6	+14.0	6.5
32.2	89.2	—10.7	+ 9.2	8.0
35.4	86.3	— 7.5	+ 6.3	5.0
39.0	85.4	— 3.9	+ 5.4	4.0
42.9	83.8	0	+ 3.8	6.0
47.2	80.4	+ 4.3	+ 0.4	5.0
51.9	75.6	+ 9.0	— 4.1	4.0
57.1	71.6	+14.2	— 8.9	5.5
62.8	69.0	+19.9	—11.0	6.5
69.1	65.8	+26.2	—14.2	6.5
76.0	64.2	+33.1	—15.8	6.5
83.6	61.2	+40.7	—18.8	6 0
91.9	58.6	+49.0	—21.4	6.5

The general course of the illusion is shown in Fig. 66. The amount by which each 80-gramme block differed from the middle size is laid off along the horizontal axis; the values are taken from column C of the table. The amount by which each 42.9 mm. blocks differed from the standard of 80 g. is laid off on the vertical axis. The arrangement is thus the same as that of Fig. 65. The amount of the illusion for any block is expressed by erecting an ordinate indicating the number of grammes by which it appeared to differ from the standard set. Thus the block —10.7 of column C, which weighed 80 g., appeared to be the same as a block of Set B weighing 9.2 g. more than 80 g.; above the point —10.7 of the horizontal axis we erect an ordinate equal to + 9.2 of the vertical scale. Proceeding thus for all the blocks we obtain the curve of

illusion as in Fig. 66. If size had had no influence, all the blocks would have been found to correspond with the weight of 80 grammes; the curve would have coincided with the horizontal axis, as is practically the case with the G curve of Fig. 68.

It is evident that there is a definite law governing the suggestive influence of size on judgments of heaviness. For the case here involved I have found[1] the law to be

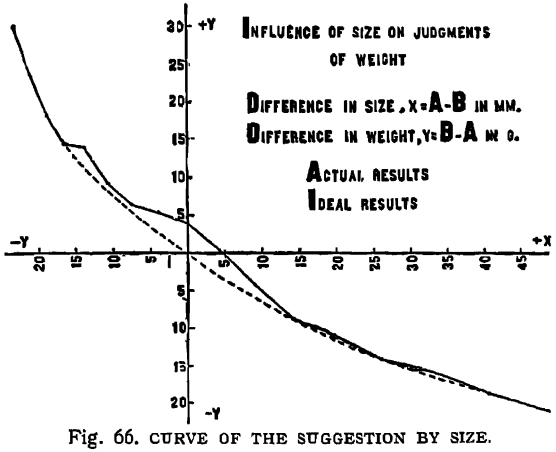


Fig. 66. CURVE OF THE SUGGESTION BY SIZE.

$i = \frac{k}{s+c} - d$, where i is the amount of illusion, s is the difference in size acting as a suggestion, c is the diameter of the blocks of constant size (B series), d is the weight of the blocks of constant weight (A series), and k is a constant depending on the nature of the experiment. Thus in these experiments c was 43 mm., d was 80 g., and k was determined by the fact that the blocks were looked at while lifted, that the subject was ignorant of the illusion, &c. When the circumstances of the

[1] Scripture, *The Law of the Size-Weight Illusion*, "Science," 1897, N.S., v. 227.

experiment are changed, as in the following cases, the factor k changes; the curve, however, still retains its character as a portion of a hyperbola, as is seen in Figs. 67, 68, and 69.

Further experiments were made concerning the persistence of the illusion. In one set of experiments the observers were subjected to careful and attentive prac-

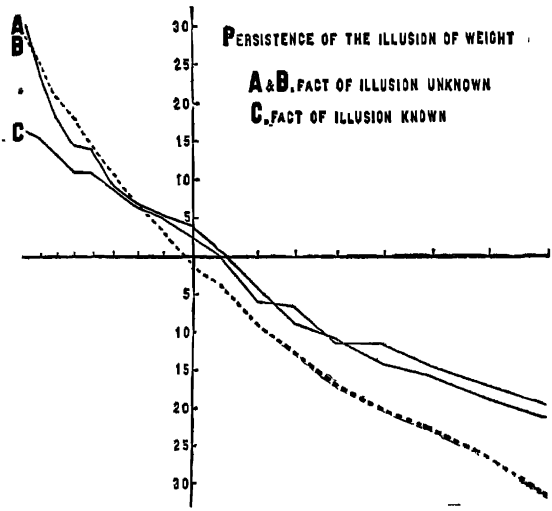


Fig. 67. PERSISTENCE OF SIZE SUGGESTION.

tice. In another set the facts concerning the blocks were told to them. The results are shown in Fig. 67. *A* is the curve taken from Fig. 66; it represents the usual course of the illusion. *B* represents the results when each observer tried the same complete experiment twenty times, under similar circumstances, making two experiments each day. No suggestion as to the degree of accuracy or success was given during the progress of the experiments. The instructions emphatically

expressed were : "The object of these experiments is to determine whether you can improve in the accuracy of this discrimination by practice. Do not allow yourself to be influenced in the least by memory of previous judgments or any theory of order or expected results." *C* is the curve when the observer knew the truth concerning the blocks, but judged them as fairly as possible.

The immediateness with which the blocks were seen

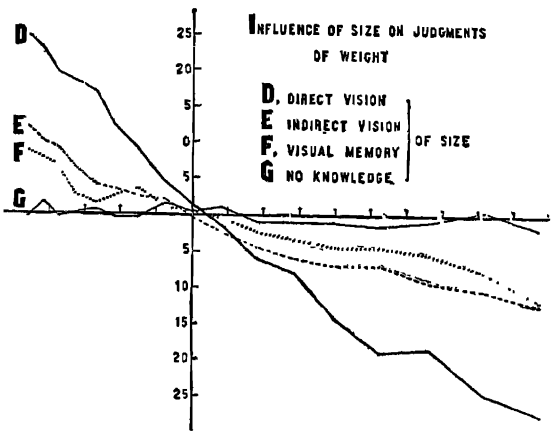


Fig. 68. SIZE SUGGESTION INFLUENCED BY DIRECTNESS OF VISION.

influences the amount of the illusion. When the observer looked directly at the blocks the illusion ran as in curve *D* Fig. 68 ; when he saw them by indirect vision the results ran as in *E* ; when he looked at them and closed his eyes the results ran as in *F* ; when he was blindfolded there was, of course, no illusion, as is shown by *G*.

This influence of expectation is derived not only from sight, but also from other senses. Seashore made

experiments in which the idea of size was acquired by different means: (1) by grasping the block round its circumference (muscle sense); (2) by feeling it laid on the palm of the hand (touch); (3) by sight; (4) by grasping it around the circumference and looking at it also. The results are shown in Fig. 69.

Finally, a knowledge, or a supposed knowledge of the material of which the blocks are made also influ-

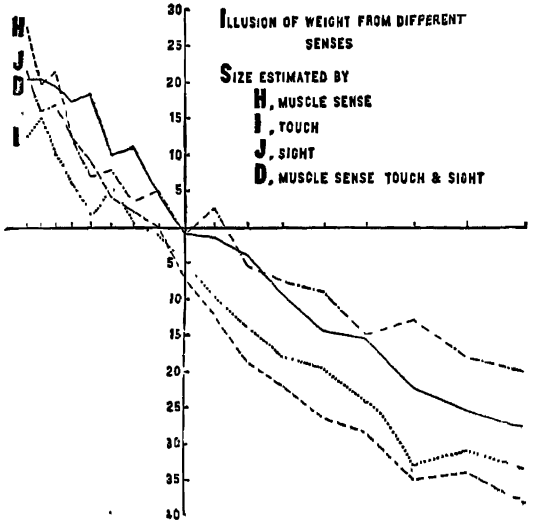


FIG. 69. SIZE SUGGESTION FROM DIFFERENT SENSES.

ences the result. Experiments made with deceptive cork, wood, and leaden weights showed over- and under-estimation according to the disappointment of the expectation.

To demonstrate such experiments before large audiences I have made a set of very large blocks. It consists of cubical wooden boxes loaded with lead to the desired weights. The small block is a cube

of 8.2 cm. and the large one a cube of 60 cm., each weighing 8¾ lbs. The standard set consists of cubes of 15 cm. ranging by ¾ lb. for each step from 2 lbs. up to 17 lbs.

Seashore tried the experiment with this apparatus on four men with the following result: The large box was matched with boxes of 5.75 lbs., 4.25 lbs., 4.25 lbs., and 2 lbs., by the respective observers—average 4 lbs.—that is, there was an under-estimation of 3 lbs., 4.50 lbs., 4.50 lbs., and 6.75 lbs. respectively—average 4.69 lbs. The small box was matched with 8.5 lbs., 14.75 lbs., 13.25 lbs., and 10.25 lbs. respectively—average 11.96 lbs.—that is, there was a corresponding over-estimation of—0.25 lb., 6 lbs., 4.50 lbs., and 1.5 lbs.—average 3 lbs. This means that mainly on account of the difference in size the observers made a difference of 2.75 lbs., 10.5 lbs., 9 lbs., and 8.25 lbs. respectively—average 7.6 lbs.—between the two boxes, which both had the same weight, 8.75 lbs. That is, the smaller is judged to weigh 2.9 times as much as the larger. This, it will be observed, slightly exceeds the average amount of the illusion between the extremes in Seashore's blocks.

This illusion is based upon the difference in volume, but a comparison between my set and Seashore's shows that it depends very much upon the shape of the weights and whether two or three dimensions are varied. The diameters of the smallest and the largest of Seashore's blocks are in the ratio of 20 : 91.9; while the one dimension of the boxes stands in the ratio 8.2 : 60, and still the illusion is not much stronger in the latter case. It is therefore evident that it does not vary directly with the volume.

In these experiments of Gilbert and Seashore "the factors that produce such a deception of judgment seem to consist in a suggestion—or, rather, a disap-

pointed suggestion—of weight. Big things are, of course, heavier than little things of exactly the same kind. When we find two things of the same appearance but differing in size, the big thing *must* be heavier. This reasoning is all done without our suspecting it, and we unconsciously allow our judgment of weight to be influenced by the size as seen. When the eyes are closed and the weights are lifted by strings, of course there is no illusion.

"Which is the heavier, a pound of lead or a pound of feathers? A pound of lead, says the unsuspecting person, and then you guy him for his stupidity. But this poor fellow, who has been laughed at for centuries, is right. A pound isn't a pound all the world over; it all depends on how the pound looks. A pound of lead is heavier than a pound of feathers. Try it with a pillow and a piece of lead pipe. No matter if the scales do say that they weigh just the same, the pound of lead is much the heavier as long as you look at it."[1]

[1] Scripture, "Thinking, Feeling, Doing," 260, Meadville, 1895.

CHAPTER XX.

PRESSURE.

THE sensations of pressure, of which we have so frequently spoken in the last few chapters, have been made the subject of special investigations.

Just as in the case of volition, we can compare our mental scale of pressure with the standard scale of weight. This has been done by Griffing[1] for several persons who gave their opinions concerning the relative heaviness of different weights. Characteristic results are shown in the figures for the two subjects.

Series of weights	2g	10g	50g	250g	1,250g	1,800g
Standard scale	1	5	25	125	625	900
Mental scale, P	1	2	6	17	93	315
Mental scale, K	1	2	4	8	26	43

The remarkable differences between the standard scale and the personal scales are apparent. These results and those of two other subjects indicate the presence of a definite law of relation with personal constants.

An important problem concerns the minimum amount of energy requisite for rousing the sensation of pressure.

[1] Griffing, *On Sensations from Pressure and Impact*, "Psychol. Rev.," 1895. Monogr. Suppl. No. 1.

A simple method of doing this is to lower small weights on the part to be tested, *e.g.*, skin, mucous membrane, &c.[1] A convenient arrangement comprises 20 to 40 small discs of cork suspended from handles by fine cocoon fibres; the discs are 3 mm. in diameter, and range in weight from 1 mg. upward by steps of 1 mg.

The usual way of performing the experiment has been as follows. The discs are lowered in succession softly on the skin until one of them is felt. The lightest one felt is the least noticeable weight: the sensation from it is said to be just above the "threshold of sensation."

The threshold of sensation, in this sense, lies, under these conditions, according to Weber, at about 2 mg. for forehead, temples, and back of forearm and hand; 3 mg. for inner side of forearm; 5 mg. for nose, hip, chin, and abdomen; 5 mg. to 15 mg. on inner surface of fingers; and 1,000 mg. on heel and nails.

In my own experience I have found that these figures are quite too low for most people. For the inner side of the hand, for example, the threshold ranges from 12 mg. to 20 mg. Some people cannot feel even the 20 mg. disc on the hand or fingers.

Tests with these discs do not afford a measurement of one of the factors involved in applying them, namely, the velocity with which they strike the skin. A disc will sometimes be felt, or not, according as it is lowered more or less gently. The energy involved in the application of the disc depends not only on the mass of the disc, but also upon the velocity of its motion.

[1] Aubert and Kammler, *Untersuchungen über den Druck- und Raumsinn der Haut*, Moleschott's "Untersuchungen," 1859, v. 145.

Measurements of the energy of the stimulus corresponding to the threshold of pressure have been made by Blix.[1]

The apparatus used comprised a straw 10 cm. long, fastened to a light horizontal axis. On the end of the straw, a piece of horsehair was fastened with wax. This was perfectly balanced by a wire. When the wire was bent sidewise over the scale, the centre of gravity was thrown out of the axis along the line of the straw. When the straw was raised to a certain height, and then allowed to fall, the hair struck the skin with an energy depending upon the position of the centre of gravity and the height of fall. The scale served to indicate the position of the centre of gravity.

The scale for the apparatus was established by determining how stiffly a spring had to be stretched in order to just yield when the horsehair struck it; and by then finding how many grammes of weight were required to make the spring yield.

Blix found that, on the most sensitive spots on the back of the hand, the threshold was at a point indicated by a pressure of 0.20 g., while on the middle of the lower arm it was 0.33, and on the thigh 1.30. These most sensitive spots he calls pressure spots. In the spaces between these spots, the threshold is very much higher. For example, at a certain group of spots on the back of the hand, the threshold was at 0.23 g., while just between these spots 1.5 g. produced no noticeable sensation.

What is this threshold? Is it the sensation from a certain weight such that all heavier weights are felt, and all lighter ones not felt? No, there is no such

[1] Blix, *Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven*, "Zt. f. Biologie," 1885, xxi. (N. F. iii.), 145.

weight; the weight that is felt in one experiment is not felt in another.

To illustrate what is meant by the threshold let us suppose the results of thousands of experiments to lie before us. Each experiment consisted in the application of weights in succession, from 1 mg. to 20 mg.

We would have records similar to the following one,

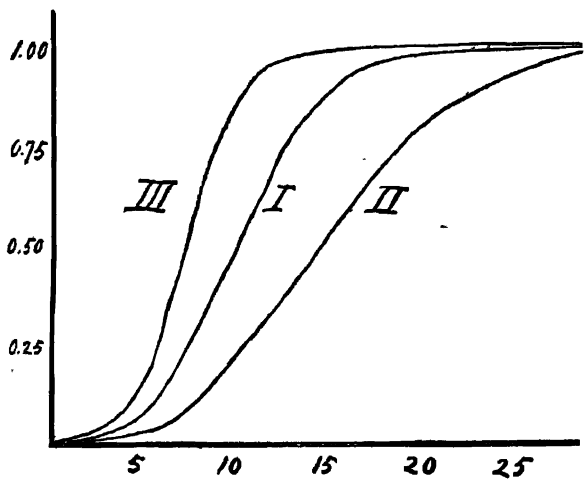


Fig. 70. PROBABILITY CURVES FOR THE PRESSURE THRESHOLD.

where a star * means that the weight was felt, and a dash — that it was not.

Weights		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Exper.	1	–	–	–	*	–	*	*	*	–	–	*	*	*	–	*	*	*	*	*	*
"	2	–	–	*	–	–	–	–	*	–	*	*	*	*	*	–	*	*	*	*	*
"	3	–	–	–	–	*	*	–	*	–	*	*	–	*	*	*	*	–	*	*	*
	⋮																				
"	11	–	–	*	–	*	*	–	*	*	–	*	*	*	*	*	*	*	*	*	*

The total number of occurrences for each weight is to be written at the bottom. On the horizontal line

in Fig. 70, we lay off the weight units in milligrammes. We then erect ordinates proportional to the number of times each was felt; as a basis for this proportion it is convenient to take 1.00, and we therefore express the percentage of times each was felt. Connecting the ends of the ordinates, we get a curve of the form I.

Each weight has been felt a certain proportion of the time. Certain extreme weights have been felt all the time. Shall we take the weight that is felt all the time as the threshold? As we increase the number of experiments, we find that this weight is liable to slowly become greater; we could not point to any particular weight as the first one that would be *always* felt as the lightest one. We assume, however, just as in Chap. II., that anything that happens 0.999988 times out of a 1.000000 is practically certain to happen; we therefore pick out this weight as practically sure to cause a sensation; it may be called the "upper threshold." In like manner we pick out the weight that fails to be felt, 0.000022 times out of 1.000000; this is the "lower threshold." There is still another value entitled to be called the threshold; this is the one corresponding to the weight that is felt half the time, or the "median threshold." This median threshold holds a peculiar position. We may conceive the threshold of sensation as really a constant quantity, and the particular values obtained in the experiments to be brought about by a multitude of interfering phenomena, which cause the results to fluctuate. The threshold is hereby treated as analogous to the true value of a quantity in physical measurements. In measuring the length of a line the results will disagree, provided the method be fine enough, as explained in Chap. IV.; we assume that there is some true value, which would be attainable with infinitely accurate methods. In a statistical problem we

assume that there is an ultimate value toward which, with finer methods and greater numbers, the quantity tends. The upper, the median, and the lower thresholds are similarly quantities toward which the 0.0022 %, the 50 %, and the 99.9978 % values tend to settle as the number of experiments increases. Aside from this there are no reasons for considering any values rather than others as the thresholds.

The three thresholds will differ under different conditions for different persons, &c. For one condition the curve may run as shown in I. (Fig. 70), for another as in II., for still another as in III. Persons or conditions whose thresholds are lower are said to be "more sensitive," and likewise the reverse.

We have every reason to believe that the curve of results for an extended series of careful experiments would follow the law for $\Phi(\gamma)$ (see Appendix I.), where $\gamma = h$ D, D being the amount of the weight and h a quantity indicating the subject's fineness of sensation. For any given subject or condition we would, therefore, know the frequency of occurrence for each value as soon as we had determined it for one of them.

In this explanation of the threshold we have supposed the number of experiments to be practically unlimited. In actual work, the number of experiments is not large. It is thus impossible to pick out directly the upper and lower thresholds. The median threshold can be picked out directly as the middle value M (p. 46). Of course, as the number of experiments is limited, there is a degree of uncertainty attached to it. This degree of uncertainty can be indicated, as in Chap. II., on the supposition of the validity of the $\Phi(\gamma)$ law for this case. On a similar supposition we can also deduce the upper and lower thresholds as follows.

The general experience of science since the time

of Gauss,[1] in dealing with fluctuations of the kind we find here, has tended to the universal justification of $\Phi(\gamma)$; we are therefore entitled to assume its validity here, until experimental evidence to the contrary is produced.

To find the upper threshold we refer to the table for $\Phi(\gamma)$. For the value occurring 50 % of the time we find $\gamma' = 0.4769$, and for that occurring 99.9978 % we find $\gamma'' = 3.0000$. If the median threshold M corresponds to γ', the upper threshold will be $\frac{\gamma''}{\gamma'} M = 6.2906\, M$; and by analogous reasoning the lower will be 0.00004 M.

A very useful practical threshold is the quartile value.[2] The over- and the under-quartiles are those values above or below which 25 % of the results lie. With a very uncertain median the quartiles will be very distant, and likewise the reverse. With $\Phi(\gamma)$ as valid for the particular set of experiments, the over-quartile Q will be equally distant with the under-quartile q, from the median M, *i.e.*, $Q - M = M - q$.

The usual method of determining the threshold differs from those we have used by stopping at the first weight felt. Thus, in the example given above, the experimenter would stop with the weights 4, 3, 5, . . ., 3 and would not use any beyond the first one felt. The results for the smallest weights are then averaged. What is meant by the value thus obtained?

If in all cases every weight were felt beyond the first one, the average for the first ones would—as a consideration of the $\Phi(\gamma)$ curve will show—be theoretically, for an unlimited number of experiments, the same as the median threshold. But the weights beyond the lightest

[1] References and psychological applications are to be found in Scripture, *On Mean Values for Direct Measurements*, "Stud. Yale Psych. Lab.," 1894, ii. 1.

[2] Galton, *Statistics by Intercomparison*, "Phil. Mag.," 1875 (4), xlix. 33.

one are not all felt. Just what is the relation between the first weight felt and the median threshold, I am at present unable to say. Theoretically, I suppose, this weight corresponds to the smallest one in a series of observations, and holds to the median threshold the relation expressed by $s = \frac{\sqrt{\pi}}{n+1} M$ where s is the average first weight, M the median threshold, and n the number of experiments. At any rate it is a questionable quantity.

The next problem in investigating sensations of pressure is that of the just noticeable difference. This is the amount of difference that can be made in a given pressure before the difference is noticed.

Most experiments have been made (1) by applying weights in succession to the same spot until two weights were found whose difference was great enough to be noticed; (2) by applying in succession weights having certain small differences, and noting the proportionate number of times in which this difference was noticed. The latter method is similar to the method of right and wrong cases described in Chap. XIX. The former method will now be described; it brings out many prominent characteristics of psychological procedure.

The experiments are generally made in the following way. Weights just alike in size are provided, but increasing and decreasing in weight regularly from a given standard. For example, from a standard of 100 g. the set will run upward and downward by steps of 1 g. The standard is applied, say, to the palm of the hand, the hand being supported on a cushion; it is removed, and, after about 2 secs. the next weight, 101 g., is likewise applied for an instant. The subject immediately states whether the second weight felt lighter than, or heavier than, or the same as the first. After a short rest the standard is again used; then the 102 g. weight is applied.

In a similar way the weights 103 g., 104 g., &c. are used, till a difference has been detected several times in succession. Then the experiments are repeated, beginning with a definitely heavier weight than the standard, and proceeding downward till the difference has remained unnoticed several times in succession. In a similar manner the weights lighter than the standard are used. The record will look like the following :—

W	d'_o	d''_u	W	d'_u	d''_u
101	=	=	99	=	=
102	=	=	98	=	<
103	=	=	97	=	=
104 ↓	=	=	96 ↓	=	=
105 ↓	= ↑	> —	95 ↓	= ↑	=
106	> — ↑	>	94	= ↑	< —
107	>	>	93	=	<
108	>	>	92	< —	<
109	>	>	91	<	<
110	>	>	90	<	<

Standard : 100 g. ; standard first.

W = weight applied.

The signs =, >, < indicate the judgments of W as compared with standard ; ⟶ indicates the order in which the weights of W were used.

The short lines indicate the limits within which the subject seems to be uncertain as to the difference. The weight, which is just at this limit is to be considered as just perceptibly different.

The difference between it and the standard is the just perceptible difference.

In the record given the just perceptibly greater difference when going away from the standard is $d'_o=6$, when going toward it $d''_o=5$; the just perceptibly smaller difference is $d'_u=8$ away and $d''_u=6$ toward. We notice that when going away from equality, the difference remains longer unnoticed. Experience proves this to be a regular phenomenon; the law governing it in its

dependence on the standard, the size of the successive differences, &c., has not been established. The just perceptibly greater difference is $d_o = \frac{d_o' + d_o''}{2} = 5\frac{1}{2}$; the just perceptibly smaller difference is $d_u = \frac{d_u' + d_u''}{2} = 7$. We cannot, however, compare d_o with d_u until we perform another experiment with the standard weight last.

The record for the second set will have this appearance:—

W	v_o'	v_o''	W	v_u'	v_u''
101	=	=	99	=	=
102	=	=	98	=	=
103	=	=	97	=	=
104	=	=	96	=	< —
105	=	=	95	=	<
106	=	=	94	< —	<
107	=	=	93	<	<
108	=	> —	92	<	<
109	> —	>	91	<	<
110	>	>	90	<	<

Standard: 100 g; standard second.

We have $\delta_o = \frac{\delta_o' + \delta_o'}{2} = \frac{9+8}{2} = 8\frac{1}{2}$ and $\delta_u = \frac{\delta_u' + \delta_u''}{2} = \frac{6 \times 4}{2} = 5$. It therefore makes a difference whether the standard is placed first or second. If we wish to eliminate this difference in order to obtain the just perceptibly greater and smaller differences, we take $D_o = \frac{d_o + \delta_o}{2} = 7$, and $D_u = \frac{d_u + \delta_u}{2} = 6$. This illustrates a generally observed fact that the just perceptible difference is less on the side of smaller than on the side of greater. If, however, we wish to determine the relation of the second weight to the first one, we remember that adding d_o to 100 g. gives the just noticeably greater weight and subtracting

d_u from 100 g. gives the just noticeably smaller weight. Thus 105½ g. and 93 g. are the just noticeably different weights; the weight lying midway between them is 99½ g. when the standard is first. When the standard is second the weights are 108½ g. and 95, and the middle weight is 101¾ g. This illustrates the general law that when two weights are compared the second is over-estimated.

The psychological processes involved in experiments on the just noticeable difference can be brought out by various methods of procedure. In fact, the chief interest of these experiments lies, not in finding a definite figure for the just noticeable difference, but in observing how this difference changes with the varying mental attitude. What is measured in such experiments is not a fact of sensation, but the accuracy of judgment, the attitude of expectation, the quality of self-reliance, &c.[1]

These facts have been generally overlooked, and the experimenters have sought to eliminate the influence of the mental attitude. Consequently, in discussing the just noticeable difference, we seldom have the necessary data for a full understanding of it.

In the records just given, the subject knows how the experiments are carried out, or carries them out himself: this is the "conscious" method, approved by Fechner.

By changing the method of experimenting we can change the subject's mental condition. A mere glance at a record sheet frequently suffices to tell what this attitude was.

Suppose we say to the subject: the weights are to be

[1] It may seem a strange statement, but it is true that in these methods we possess the means of measuring such apparently inaccessible mental processes as faith, honesty, and the like. The proper development of the methods is still in the future.

given you in regular order, but you will not know whether they will run toward greater or toward less. We get a record of this sort where, for brevity, only d_o' and d_u' are given :—

W	d_o'	W	d_u'
101	=	99	=
102	<	98	>
103	=	97	<
104	>	96	=
105	=	95	=
106	<	94	< —
107	> —	93	<
108	>	92	<
109	>	91	<
110	>	90	<

We see at once the effect of uncertainty as to which way the weights are to change. This is called the "partly unconscious" method.

Suppose, again, we say : the weights are to be given you in an utterly irregular order. Then, if we actually carry them out in regular order, unknown to the subject, we get a record where the mind is in an unprejudiced condition. The judgments will be more irregularly distributed ; for example :—

W	d_o'	W	d_u'
101	<	99	>
102	>	98	<
103	=	97	=
104	<	96	>
105	=	95	=
106	> —	94	=
107	>	93	< —
108	>	92	<
109	>	91	<
110	>	90	<

This is the "unconscious method." The values for the

just perceptible difference may be the same as those with the partly conscious method; they will be larger than with the conscious method. The decrease of the values from the just perceptible differences in the conscious, as compared with the unconscious, method gives us the effect of the mental attitude of confident expectation.

Let us now consider the relation between the method of finding the just perceptible difference and the method of right and wrong cases explained in the previous chapter.

Within the doubtful region marked off by the short lines, the values with the unconscious method are very irregular. The various influences that hinder us from detecting small differences are continually changing. If, instead of changing the weights in the last experiment, we always give to the unsuspecting person the standard of 100 g., we get a record of this character: =, <, <,=, >, =, = <, >, >, =. We have here passed to the method of right and wrong cases. We get a number of judgments in which the first weight is judged to be >, others in which it is =, and others in which it is < the first.

Now, suppose that we use the two weights 100 g. and 101 g., the subject will, in a long series of experiments, show an excess for >, with less for <, together with a certain number of =.

If 102 g. be used, the differences will be still greater, and, as the weight compared with the standard becomes 103 g., 104 g., and still more different, the > preponderate still more at the expense of = and <, till finally all judgments are >.

Instead of carrying out the experiment with successively different weights, as just described, we might select a certain pair of weights, say, 100 g. and

105 g., and determine the relative proportions of >, =, and <. This is the method which has been called the method of right and wrong cases. It is evident that, if one person is more sensitive than another, he will give more > judgments, or "right cases."[1]

We must now turn to a new psychological quantity; the least noticeable, or just perceptible, change in pressure.

Experiments on the just perceptible change in pressure from 0, *i.e.*, on the least perceptible pressure, have been made by Hall and Motora,[2] who used a little car running along a scale-beam at definite rates. The pressure was thus regularly increased or decreased from any desired initial pressure. The following is a characteristic result. With a rate of change of $\frac{4}{125}$ per second, the just perceptible change (average of increase and decrease) was found for Motora to be—

Initial weight	5	10	20	30	40	50	60	65	70	75	100	200	500	
Just perceptible change	3 5	5 15	7 8	7 9	13 3	13 0	14 0	14 5	15 4	17 5	27 9	54 4	163 4	
Ratio	1 7	1 5	1 4	1 3	1 3	1 3	1 2	1 2	1 2	1 2	1 3	1 3	1 3	

The just perceptible change was thus almost exactly a constant fraction of the initial weight.

Starting with an initial weight of 50 g., the just perceptible changes with different rates for Motora were—

Rate...	$\frac{16}{125}$	$\frac{8}{125}$	$\frac{4}{125}$	$\frac{2}{125}$	$\frac{1}{125}$	$\frac{1}{250}$	$\frac{1}{500}$
Just perceptible change	22.0	14.6	13.0	9.3	9.1	6.8	6.6
Ratio	1.4	1.3	1.3	1.2	1.2	1.1	1.1

There is thus a decrease in the amount of the just perceptible change as the rate was made slower.

[1] The rules for calculating the results obtained by this method are to be found in Bruns, *Ueber die Ausgleichung statistischer Zählungen in der Psychophysik*, "Phil. Stud.," 1893, ix. 1. See p. 268.

[2] Hall and Motora, *Dermal Sensitiveness to Gradual Pressure Changes*, "Am. Jour. Psych.," 1887, i. 72.

The problem has been taken up for further treatment[1] by Dr. C. E. Seashore, of the Yale Laboratory. His apparatus is constructed on the principle that a body immersed in water decreases in weight in proportion to the extent to which it is immersed. A light and carefully balanced scale-beam has a brass rod suspended on each end. On the lower end of one rod stimulating points of any size may be placed; the other rod hangs inside of a large glass tube, in which a column of water may be raised or lowered at any desired rate. At the lower end of the glass tube is a U-shaped tube in which nozzles of various sizes may be inserted to regulate the size of the flowing stream. Water is conducted through a hose from a reservoir on an upper floor. The head of the stream being kept constant, the rate of flow is regulated by the size of the nozzles. As the immersed rod becomes lighter owing to the rise of the water, the other rod becomes proportionately heavier. The height of the column of water is read off on a mm. scale, and the amount of change in pressure may easily be computed from the known amount of water displaced by the rod. This apparatus solves the problem of getting a gradual change of pressure at any desired rate without jarring the stimulating point.

The first series of experiments was made to find the least perceptible change for various rates of increase. The spot investigated was the outer side of the index finger at a point midway between the second and the third joints, with a circular area of stimulation 5 mm. in diameter. The initial stimulus was 5 g. Hence the only varying quantity was the rate of increasing the

[1] The account of the extensive investigations of Stratton, *Ueber d. Wahrnehmung v. Druckänderungen bei verschiedener Geschwindigkeiten*, "Phil. Stud.," 1896, xii. 525, was received too late for consideration here.

pressure. The method of experimenting was as follows: The observer having adjusted his hand on a special hard rubber support, by which the index finger would be kept still and yet be free from any surrounding contact, the stimulating point (having a pressure of 5 g.) was placed on the selected spot as gently and quickly as possible after a signal. About two seconds after the point had been placed a second signal was given, and the stimulus began to increase. The observer knew nothing about what rate was to be chosen in any trial; he was simply asked to give a signal when he was sure the stimulus had increased. Five rates were used, and the trials were so distributed as to eliminate the effect of fatigue in summing up the results for comparison.

TABLE.

A	0 036	0.220	0.570	0.965	1 326
B	3.2 (1.8)	6.5 (2.4)	9.4 (2.8)	10.6 (2.4)	12.1 (2.9)
C	1 7 7	5.9	3.2	2.2	1.8

The table gives the results[1] of the experiments on thirteen observers, being the averages from ten trials by each observer on each rate. Thus each figure in the record represents 130 single trials. The figures in the horizontal line A give the part of the initial stimulus by which the pressure increased per second; B gives the number of grammes to which it was necessary to increase this stimulus in order that the increase should be felt. The mean variations are indicated by the figures in parentheses. C indicates the number of seconds during which the stimulus increased before the change was perceived. These results are represented in Fig. 71, where the amount of increase per second is marked off proportionally on the abscissa. It also represents the proportional part which the increase per second

[1] To be found in detail in "Stud. Yale Psych. Lab.," 1896, iv.

was of the initial stimulus. The amount of increase in grammes is indicated on the vertical axis.

The previously noticed law is here confirmed. Within the region here investigated the sensitiveness to change increases as the rate decreases. But this is true only for moderate rates. It is interesting to note how the curve must be deflected at both ends if continued. The slowest rate here used is the slowest rate by which the pressure can be increased and yet be detected by the observer. From other experiments we may infer

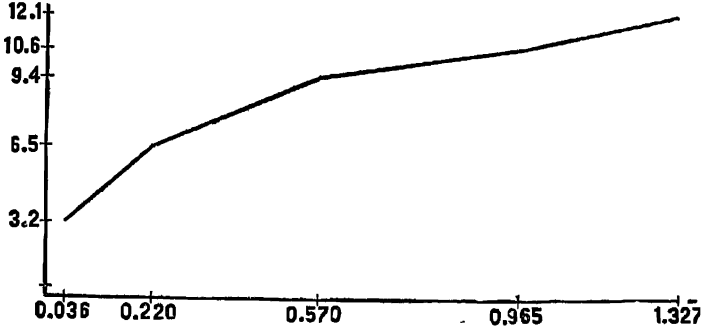


FIG 71. DEPENDENCE OF THE LEAST NOTICEABLE CHANGE ON THE RATE OF CHANGE.

that in the short distance between rate 0.036 and 0 the curve must rise abruptly to a very high or indefinite point, *i.e.*, when the increase in pressure is sufficiently slow it may be continued indefinitely, or until it becomes painful without a distinct sensation of pressure. The fastest rate is the fastest rate that can be used in accurate measurement by the present method. To investigate the variation with the rate between this rate and instantaneous increase of the pressure, Dr. Seashore has constructed two compound scales, by means of which very rapid rates of increase may be produced and

measured, and by the other an instantaneous increase of the initial stimulus may be made without taking off or placing any weights on the stimulating point. Sufficient results have not been obtained on the former point to allow any generalisation, but for instantaneous addition of weight to 5 g. two seconds after it was applied seven of the thirteen observers in the previous experiment perceive on the average 0.35 g. This amount is indicated by the mark over the dotted line in the curve, which shows that the curve must descend abruptly from the highest point to this, the lowest. These two abrupt deflections of the ends of the curve in opposite directions show how important and complex the one factor of the rate of change in pressure may be.

How great the just perceptible change can be made to become by making the rate of change extremely slow is a matter that still remains for investigation. It is worthy of note that it has been found possible in 5½ hours to actually crush a frog's foot, without a sign that the pressure was felt, by screwing down a button at the rate of 0.03 mm. per minute. A similar experiment showed that a live frog can actually be boiled without a movement if the water is heated slowly enough; in one experiment the temperature was raised at the rate of 0.002° C. per second, and the frog was found dead at the end of 2½ hours without having moved.[1] If a frog can be crushed or boiled without any evidence that he has noticed it, it is at least an interesting

[1] The literature on these experiments with frogs includes Heinzmann, *Ueber die Wirkung sehr allmäliger Aenderungen thermischer Reize auf die Empfindungsnerven*, "Archiv f. d. ges. Physiol." (Pflüger), 1872, vi. 222. Fratscher, *Ueber continuirliche und langsame Nervenreizung*, "Jenaische Zeitschrift," 1875, N. F. ii. 130. Sedgwick, *On the Variation of Reflex Excitability in the Frog induced by changes of Temperature*, "Stud. Biol. Lab., John Hopkins Univ.," 1882, 385.

question of what can be accomplished in this direction with human beings.

This whole question of the just perceptible change and its dependence on the rate has just begun to receive recognition as one of the most fundamental questions of psychology. The general problem can be stated in the following way. Starting with the initial stimulus r (pressure, tone, light), we cause one of its properties, i (intensity, extent), to change at the rate of $u = \frac{di}{dt}$. The just perceptible change will be $D = f(r, \frac{di}{dt})$.

This is the problem of the law of the just perceptible change [1]; its particular form remains to be determined. It is a fundamental law of mental life and has far-reaching consequences.[2]

Finally, there is another mental quantity closely related to the just perceptible change, namely, the just perceptible acceleration of the change. Suppose the pressure in the experiments mentioned above to be steadily changing at a noticeable rate; how great an acceleration or retardation in this change is required in order to be noticed. This acceleration—to continue the example just given—is $u' = \frac{d^2i}{dt^2}$, and its law would be given by the determination of $A = f(r, \frac{di}{dt}, \frac{d^2i}{dt^2})$.

I have observed this quantity A in experiments on the pitch of tones,[3] but have not yet been able to measure it. No data are at hand concerning the just perceptible acceleration for pressure.

[1] Scripture, *The Method of Minimum Variation*, "Am. Jour. Psych.," 1892, iv. 577. Ibid., *Ueber die Aenderungsempfindlichkeit*, "Zt. f. Psychol. u. Phys. d. Sinn." 1894, vi. 472.

[2] Preyer, *Die Empfindung als Funktion der Reizändering*, "Zt. f. Psychol. u. Phys. d. Sinn.," 1894, vii. 241.

[3] Scripture, *On the least Perceptible Variation of Pitch*, "Am. Jour. Psych.," 1892, iv. 579.

CHAPTER XXI.

PAIN.

PAIN has been asserted to be a special kind of sensation, or a noticeable degree of the obscure sensations from the bodily tissues, or the exaggerated degrees of all sensations, or the extreme degree of dislike, &c.

We must here, however, treat the not very extensive collection of experimental data in a manner independent of theories.

As the intensity of a sensation is increased from zero upward it generally reaches a point at which it becomes disagreeable (or agreeable), and still further onward a point where it becomes painful. The relations between the threshold of sensation, the threshold of disagreeableness, and the threshold of pain, can be illustrated by the following experiment:

The current from a battery runs through the primary circuit of an inductorium with its vibrator; the cords from the secondary circuit end in two electrodes, a large one to be held in the hand, and a small one to be applied to the point of the finger. The secondary coil is started at some distance from the primary, and is slowly moved toward it. Instructions are given to the subject to say, (1) when he first feels a sensation from the electrode; (2) when the sensation first becomes disagreeable; and (3) when it first becomes painful. The experimenter records the position of the secondary coil, and consequently the strength of the current at each of these points. The reader has probably made to himself the remark usually made by persons before beginning the

experiment: namely, that it will be easy enough to tell when the sensation is first felt, and also when it becomes painful, but that disagreeableness is too indefinite to be determined. Let us suppose, however, that ten experiments have been made on such a person; we have the means of obtaining a numerical expression for this indefiniteness—namely, by finding the mean variation.

The following is a characteristic average of ten experiments on one subject: threshold of sensation, 105, MV 4.2; threshold of dislike, 95, MV 3.7; threshold of pain, 82, MV 4.2. The figures indicate the number of millimetres by which the secondary coil was distant from its most effective position. Experiments on over twenty students gave very similar records; the mean variation was generally about the same fraction of the threshold in all three cases. The conclusion is amply warranted that "dislike" is as clear and definite a result of a stimulus as sensation or pain. The further consideration of the feelings of dislike and liking will be found in the following chapter.

Experiments have been made on pains produced by pressure.

The pressure algometer consists essentially of a strong spring by means of which a rubber disc or point is pressed against the surface to be tested.

Some experiments of this kind made in New York City gave the following results in kilogrammes[1]:

Subjects	50 boys.	40 College students (men)	38 law students	58 women	40 college students (women)
Ages	12 to 15	16 to 21	19 to 25	16 to 22	
Average	4.8	5.1	7.8	3.6	3.6

[1] Griffing, *On Sensations from Pressure and Impact*, 14, "Psych. Rev.," 1895, Suppl. 1.

The question of the relative sensibility of men and women to pain is of interest in connection with the greater power of recovery from injury that is found in women. The facts reported have been summarised by Ellis.[1]

Sensations of pain, as well as of pressure, can be produced by the impact of falling bodies. A box containing a weight is allowed to fall upon the palm of the hand. It is guided by grooves without appreciable friction; the part that strikes the hand is 1 cm. in diameter. Experiments by this method[2] gave for one observer an average 1,670 gramme-centimetres, and for another observer 935 gramme-centimetres as the energy necessary to just produce pain.

At this point we must leave the subject of pain. Practical acquaintance with its various forms and complications is a part of the duty of every physician; but a scientific study of its psychological laws has only been begun.[3]

[1] Ellis, *Man and Woman*, 120, London, 1894.

[2] Griffing, as before, p. 55.

[3] A summary of the present knowledge concerning pain is to be found in Goldscheider, "Ueber d. Schmerz," Berlin, 1894.

CHAPTER XXII.

FEELINGS.

UNDER the head of "feeling" we shall consider the two mental states which we express as "dislike" and "liking," or "disagreeableness" and "agreeableness." The various views concerning these phenomena: that they are properties of sensations like intensity and quality, that they are separate sensations, that they are unconscious reasonings, that they are relations between ideas, &c., are not, in the present state of our experimental knowledge, proper subjects of discussion here.

It is the duty of the psychologist to present the thoroughly proven data concerning "dislike" and "liking" as far as possible, and to suspend judgment concerning the various theories until further data are obtained.

In the previous chapter an experiment was described wherein it was shown that an electrical stimulus at a certain intensity aroused a sensation of touch, that as it increased in intensity a feeling of disagreeableness was added, and that beyond a still higher point a sensation of pain was also added. Similar experiments might be —but have not been—made on the various stimuli that produce disagreeableness and pain.

The question now arises concerning agreeableness.

Agreeableness is connected with disagreeableness by

many facts of every-day life, by language, &c., nevertheless they are two distinct phenomena. The electrical stimulus described may not at any intensity become agreeable, although at a certain intensity it does become disagreeable. On the other hand, another stimulus, *e.g.*, a colour from the spectrum, may, as its intensity is increased, become agreeable at a certain point and remain so until it becomes painful. Again, there are certain sensations that are always agreeable. Finally, there are portions of the body to which the application of an object produces simply and solely a feeling of agreeableness without any sensation of pressure.

The method for measuring disagreeableness and agreeableness has not yet been found. All that we can do is to pick out what stimuli are accompanied by agreeableness or by disagreeableness, and also to pick out those that are most agreeable or disagreeable.

According to general observation bright colours, clear sounds, certain tastes, odours, and touches are agreeable, while dull colours, harsh sounds, other tastes, odours, and touches are disagreeable. Moreover, various forms and combinations of colour, as in architecture, painting, and decoration; various combinations of sounds, as in music; and various combinations of odours and tastes, as in a well-arranged dinner, are considered agreeable, while others are not.

The first to attempt to obtain systematic data on what are the most agreeable subjects was Fechner.[1]

Experimental æsthetics, as these investigations have

[1] The following account of his work is taken—as far as possible in the original words—from Fechner, *Zur experimentalen Aesthetik*, "Abhdl. d. k. sächs. Ges. d. Wiss., math.-phys. Cl.," 1871, ix. 555; Ibid., "Vorschule der Aesthetik," ch. xiv., Leipzig, 1876; Ibid., *Wie es der exper. Aesthetik seither ergangen ist*, "Im neuen Reich," 1878, Nos. 28, 29.

been called, arose from discussions concerning the "golden cut." The "golden cut" is an expression of the relation between two quantities whereby the smaller bears the same relation to the larger as the larger bears to the sum of both, *i.e.*, as 1 to 1.618. This relation has been asserted to be the fundamental law in the divisions of the human form, in the proportions of the higher animals, in the construction of the plants, in the forms of various crystals, in the arrangement of the planetary system, in the proportions of the most beautiful productions of architecture, sculpture, and painting, in the most satisfying accords of musical harmony and, finally, in the construction of a great universal comparative science of nature.[1]

Is the "golden cut" really the most agreeable relation for forms? this is the question that Fechner asked. To answer it he used three methods. For the "method of choice" he placed thin rectangles of white cardboard with a constant area before many persons and required each subject to point out which impressed him as the most agreeable one, and which as the most disagreeable one. For the "method of application" he measured the most various rectangular objects to be found in daily life where the relation between the parts was not determined by the use, *e.g.*, visiting cards, paintings. For the "method of production" the subject drew figures in the proportions most agreeable to him.

Fechner's results have been briefly stated by himself as follows:

1. The æsthetic value of the "golden cut" has been much exaggerated, but nevertheless it is to be recognised as valid under certain limitations.

2. That for the sides or dimensions of rectangles it

[1] Zeising, "Das Normalverhältniss der chemischen und morphologischen Proportionen," Leipzig, 1856.

has an indubitable precedence before all other relations, but yet not so emphatically before relations that differ slightly from it.

3. That the "golden cut" is decidedly at a disadvantage as compared with the relation of 1 to 1 for the division of horizontal objects and of 1 to 2 for the relation of the upper to the lower part of crosses.

An example of the modification of forms in the tendency toward this relation is found in the grave-crosses (instead of grave-stones) of Germany and in ornaments in the form of a cross. The model for all these is the crucifix. Fechner's measurements of jewellers' crosses showed that the cross-piece of the crucifix stands too high to produce a pleasing division between the upper and the lower parts of the upright and that the relation in the ornamental crosses was as 1 to 2. In the grave-crosses the crosspiece stood a little higher than in the ornamental crosses, but still not so high as in the crucifixes.

These experiments by Fechner remained the only ones until the problem was taken up by Witmer, who made use of an improved method of choice.[1] It was found possible to establish the main points of the curve of æsthetic pleasure for simple forms. For example, a series of rectangles was submitted to a subject with the demand as to which were the most displeasing ones, which the most pleasing, which the apparent square, which the figures just as pleasing as the apparent square, &c. Experiments on seven persons gave, for the rectangle, closely agreeing results. If on the X axis we start with a true square and suppose this to gradually lengthen, we can indicate the curve of resulting æsthetic pleasure as in Fig. 72. The real square 1 : 1 is

[1] Witmer, *Zur experimentellen Aesthetik einfacher räumlicher Formverhältnisse*, "Phil. Stud.," 1893, ix. 96, 208.

disagreeable, the apparent square 1 : 1.030 is very agreeable; the most agreeable figure is 1 : 1.651; the figures just as agreeable as the apparent square are 1 : 1.452 and 1 : 2.201; the most disagreeable figure is 1 : 1.181 and the next most disagreeable one is 1 : 3.090.

These and similar experiments on crosses, ellipses, &c., showed with substantial agreement that the most agreeable figure is better expressed on an average by 1 : 1.65 than by the "golden cut" 1 : 1.62. There is no reason for preferring the "golden cut" to the empirically established relation unless a mathematical mysticism be such. We can therefore accept 1 : 1.65 as in general the best value.

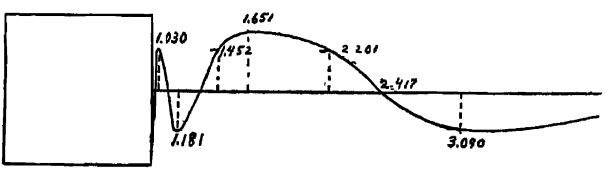


Fig. 72. CURVE OF AGREEABLENESS FOR RECTANGLES.

The investigations of Fechner and Witmer were followed by Cohn's experiments on the feelings connected with colours.[1]

Pure, brilliant colours were obtained from light transmitted through various combinations of coloured gelatine. The colours appeared in pairs as small rectangles in the wall of a darkened chamber. The subject stated which of two colours or combinations produced the stronger feeling of agreeableness. From the results it was possible to determine the relative degrees of agreeableness of the various colours and their combinations. The conclusions were drawn from experiments on fifteen

[1] Cohn, *Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihrer Combinationen*, "Phil. Stud.," 1894, x. 562.

subjects. With colours of the same purity and brilliancy the relative agreeableness of one colour as compared with another seemed to be a purely individual matter.

The law of agreeableness for pairs of colours can be expressed in the following manner. If the colours be arranged in a circle with complementaries (pairs of colours that in certain proportions produce white) at the ends of diameters, a combination of two colours increases in agreeableness as the colours are chosen further apart, the maximum agreeableness appearing for complementary colours. This is expressed in Fig. 73, in which the circumference of the circle is supposed to

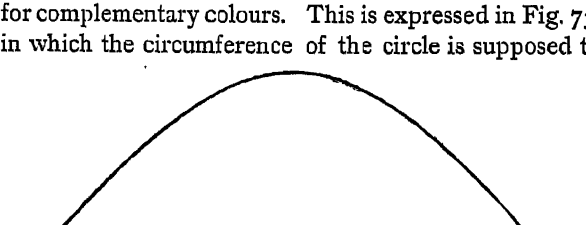


Fig. 73. CURVE OF AGREEABLENESS FOR PAIRS OF COLOURS.

be rolled out into a straight line. One of the colours is supposed to be stationary at 0°; the curve of agreeableness rises as the other colour changes to more distant hues, reaches a maximum for 180°, and sinks as the second colour again approaches the hue of the first.

Experiments with colours mixed with white (light grey) and black (dark grey) showed that of two shades of the same colour the purest one was preferred.

As for combinations of greys, the results were the more pleasing as the greys became more different. This held good as far as the limits of the experiment extended, namely, for white cardboard combined with black paper, the relative intensities being as 40 to 1.

The general law seems to be: the greater the differences between colours the greater the feeling of agreeableness. "Of course this is valid simply as a matter of sensory pleasure without secondary factors. Now the preference for strongly contrasting combinations is considered to be in general the mark of the more savage nations, and of the uncultured classes of society. When we find the same preferences among cultivated persons whose eyes have been accustomed to dull colours and small contrasts in clothing, &c., we have a passable justification for considering the rule as a general one. Former philosophers and æsthetical theorists were inclined to consider the highest ideals of beauty as universal for mankind, whereas the sensory pleasures were regarded as fluctuating and dependent on individual caprices. Is not our psychological knowledge leading us gradually to the opposite result? It seems entirely plausible that the sensory constitution of man is a fundamental and original one, whereas those complicated mental relations, on which higher æsthetical pleasure rests, vary with race, civilisation, and culture. But we have here already left the domain of facts and have anticipated future investigations."[1]

It is a noteworthy fact that, in contradiction to these psychological laws, to the natural tastes as exhibited by children and by the southern nations, we people of the English race decorate our towns, our homes, and our persons with the dullest combinations we can find. Any one who attempts to put a little life into our colours is decried as an uncultured being. As Ruskin says: "The modern colour enthusiasts who insist that all colours must be dull and dirty are just like people who eat slate-pencil and chalk and assure everybody that they are nicer and purer than strawberries and

[1] Cohn, as before, p. 601.

plums. The worst general character that decorative colouring can possibly have is a prevalent tendency to a dirty yellowish green, like that of a decaying heap of vegetables. It is distinctively a sign of a decay of colour appreciation." We may remark in passing that the instinctive love of the child for bright colours is, in many American schools, systematically subjected to a process of deformity-making till he learns to prefer the dull and dirty combinations.

With the facts related in this chapter our experimental knowledge of the psychology of feeling comes to an end. As for the mental states known as "emotions," *e.g.*, fright, joy, &c., which are closely connected with strong feelings, we have no accurate knowledge whatever. The effects of various emotions on the circulatory system have been investigated by Mosso and others; but the methods have not been arranged so as to obtain any facts concerning the *psychology* of the emotions beyond ordinary qualitative knowledge. The sphygmograph, plethysmograph, pneumograph, sphygmanometer, and galvanometer may offer great possibilities for the analysis of the confused mass of mental phenomena known as the emotions; but to attain these possibilities the investigations must be pursued according to psychological methods. These methods—*e.g.*, establishing scales of emotional intensity, finding the just perceptible difference in an emotion, &c.—have not yet been found.

CHAPTER XXIII.

SOUND.

THE innumerable sounds that we hear are by common consent roughly classed into noises and tones. The tones include sounds such as those from most musical instruments; the noises include such as rasping, hissing, banging, &c. This division cannot be carried through in all cases; the passage from tones to noise is gradual, many noises have the character of tones and likewise the reverse. Many sounds are considered as noises or as tones according to the relations in which they are found. The various blocks in a xylophone appear as noises when struck separately, but as the tones of a tune when struck in appropriate time and succession. The tones of a piano produce a noise when struck in a jumble together. Properly speaking, "noise" is a convenient term applied to such sounds as are not considered to be tones.

In the first place, let us consider a tone, *e.g.*, from a violin string when the string is snapped. If the finger or a brush is applied to the middle of the string while it is vibrating, the original sound disappears, but a fainter one an octave higher is heard. In a similar manner still higher tones can be heard by touching the string at $\frac{1}{4}$, $\frac{1}{5}$, &c., of its length. If the string be now set in vibration, the person listening will hear the violin

tone as composed of one loud tone with faint higher tones. These higher tones are the so-called overtones. Physically they are produced by the string vibrating in sections in addition to its total vibration; psychologically they are, with the main tone, simply components of the violin tone. Such a tone is a complex one. If we search for simple tones, that is, those in which only one component is found, we find them best in the tones of carefully made tuning-forks.

Simple tones have three properties: pitch, intensity, and duration. Tones have thus three dimensions; the common statement that "tones have one dimension and lie in a line" arises from the fact that in most musical instruments the property of pitch is represented by a keyboard, while the two other properties are varied by the action of the performer.

The property of pitch can be illustrated by running the voice from low to high notes, by sliding the finger up a violin string, &c. The property of intensity is that which we vary when we sing more or less loudly. The property of duration is that which characterises a tone maintained for a longer or a shorter time. In regard to pitch it must not be thought that the terms "high" or "low" have any reference to space. They might just as well be reversed, so that base tones would be called high; or utterly different names might be used. In fact, the Sanskrit terms meant "loud" and "soft"; the Hebrew meant "audible" and "deep." The Greek terms were "sharp" and "heavy," and also—referring to the strings of the lyre—"low" and "high" in exactly the opposite meaning to ours. The Latin was simply a translation of the Greek words into "acute" and "grave"; and the modern Romance languages, like the French, retain the Latin terms in modified forms. In the Middle Ages it was customary to speak of

ascending and descending; it is from this that German and English probably derive the highness and lowness of tones.[1]

Pitch is continuous. Starting with the finger at a certain place on a violin string, we can change the pitch of the tone continuously by sliding it one way or the other.

As usual, we first look for the two different quantities: the least perceptible change and the least perceptible difference in pitch.

Following some preliminary observations,[2] the least perceptible change has been investigated by Stern.[3] The tone is produced by a current of air blowing over the mouth of a bottle. The change in pitch is brought about by a current of water flowing in at the bottom of the bottle and thus, by changing its length, raising its pitch. It is necessary that this change in pitch shall proceed at some definite rate; this is accomplished by means of a variator attached to the bottle as shown in Fig. 74. The water runs at a definite rate into the variator instead of directly into the bottle, because the pitch of the tone does not rise proportionately to the rise of the water in the bottle. The variator receives a carefully determined form, such that the rise of the water in the bottle produces an even rise of pitch. The various rates at which the tone is altered are produced by nozzles of different sizes in the tube from which the water enters.

The experiment was performed as follows: the tone

[1] Stumpf, "Tonpsychologie," i. 189, Leipzig, 1883.

[2] Scripture, *On the Least Perceptible Variation of Pitch*, "Am. Jour. Psych.," 1892, iv. 580. Ibid., *Ueber die Aenderungsempfindlichkeit*, "Zt. f. Psych. u. Phys. d. Sinn.," 1894, vi. 472.

[3] Stern, *Die Wahrnehmung von Tonveränderungen*, "Zt. f. Psych. u. Phys. d. Sinn.," 1896, xi. 1.

was turned on and then gradually raised in pitch until the subject detected the change. The tone used was one of 400 complete vibrations per second. The results ran generally in a manner similar to the following specimen: with a rate of change of 0.40 vibrations per second the just perceptible change was 2.70 vibrations, with 0.50 it was 3.50, with 0.58 it was 4.12, and with 0.77 it was 5.86. The results from the various observers clearly prove that the least perceptible change under these particular circumstances increases as the rate increases. Further experiments on tones of different

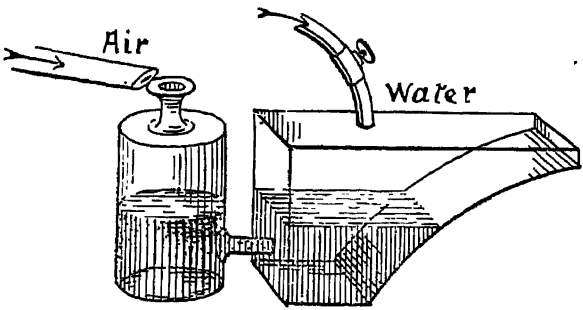


Fig. 74. THE TONE-VARIATOR.

pitch and on the least perceptible change in intensity, are to be awaited with interest.

Still another question in regard to tone-changes, namely, as to the just perceptible acceleration of the change, must remain, apart from a single observation as to its existence,[1] unanswered. Possibly by using two variators in succession in Stern's apparatus the answer might be found.

Quite a different problem from that of the just per-

[1] Scripture, as cited, "Zt. f. Psych. u. Phys. d. Sinn.," 1894, vi. 473.

ceptible change is that of the just perceptible difference; the general characters of these two phenomena have been illustrated in Chap. XX. In the present case a tone of a certain pitch is first produced, and then another of a slightly different pitch; the subject states his judgment as to whether the two tones are the same or different. An apparatus for this experiment consists of two forks of the same pitch, with a small weight at the middle of one prong of each fork. Starting with the weights at the middle, whereby both forks give the same tone, one of the weights is moved upward and downward by successive steps in the manner described for pressure. The forks are sounded alternately. The figures for the just perceptible difference have the meanings attributed to them in Chap. XX.

The dependence of the just perceptible difference on the pitch of the tone follows the general rule[1] that the just perceptible difference expressed in vibrations, is smallest with low tones and largest with high tones without the difference being very great, and that within the range of the tones usually employed in singing it remains practically constant. It thus differs completely from the law of proportionality known as Weber's law.

A convenient apparatus for rapid experiments on the just perceptible difference, is found in the tone-tester, Fig. 75. It consists of an adjustable pitch-pipe B fastened to a plate A. To the regulating rod C a long arm D is fastened, which is moved by the handle E. As C is moved inward the tone of the pitch-pipe rises. As it is moved outward the tone falls. Each movement makes a change in the position of the pointer. The tone-tester is compared beforehand with a carefully tuned piano to determine the position of the pointer

[1] Luft, *Ueber die Unterschiedsempfindlichkeit f. Tonhöhen*, "Phil. Stud.," 1888, iv. 511.

when the pipe gives A of concert pitch. This position is marked at A 435 in the illustration. The figures mean that at this point the whistle makes a tone of 435 vibrations per second. In the same manner the succeeding notes are settled. The spaces are then subdivided by the eye into thirty seconds of a tone.

To make the experiment the pointer is placed at A and the pipe is blown for an instant. The pointer is

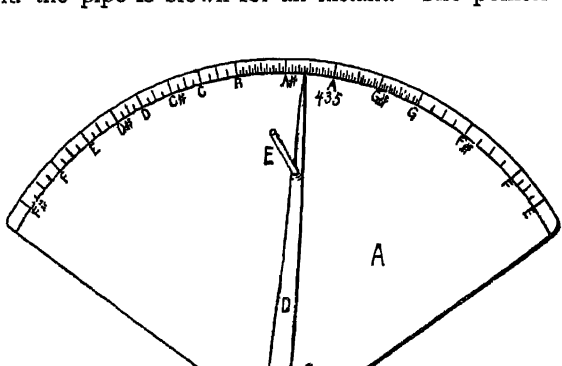


Fig. 75. THE TONE-TESTER.

then moved upward one mark and after about two seconds the pipe is again sounded. The person experimented upon tells if he hears a difference. The experiment is repeated, starting with A every time and moving one step further for the second tone, till a difference is heard. In a similar manner the difference below A is found.

The just perceptible difference between tones has been found to change with age. Experiments on

school children have resulted in a curve of the kind shown in Fig. 76.[1]

In singing, tones are produced by muscular efforts, whereby the vocal cords are stretched. Higher tones require greater tension than lower ones. In order to produce a tone of a given pitch the muscular effort put forth must be of a definite amount; any failure to get the amount correct makes itself apparent in the lack of

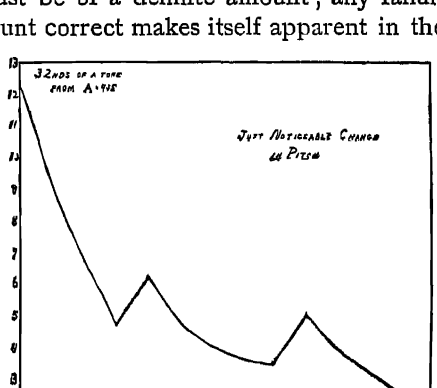


Fig. 76. SENSITIVENESS OF SCHOOL CHILDREN TO TONE-DIFFERENCES.

correspondence between the pitch of the tone intended and that of the tone produced.

Experiments on the accuracy with which tones are produced by the voice have been made by Klünder.[2] Fine metal points attached to delicate membranes recorded on the smoked drum both the vibrations of an

[1] Gilbert, *Experiments on the Musical Sensitiveness of School Children*, "Stud. Yale Psych. Lab.," 1893, i. 80.

[2] Klünder, *Ueber die Genauigkeit der Stimme*, "Arch. f. Physiol." (Du Bois-Reymond), 1879, 119.

organ pipe and those of the voice. The subject of the experiment was to sing the same tone as the organ pipe. A comparison of the two sets of waves drawn by the points showed how accurately the voice agreed with the organ pipe.[1]

A characteristic record for the tone G = 96 complete vibrations showed that the voice responded with a tone of 96.34 vibrations with a mean variation of 0.18 vibrations, the constant error being, $96.34 - 96 = 0.34$.

Of these quantities the constant error may be due to an error in the sensation of the tone or to a defective adjustment of the execution to the sensation. As the person hears his own voice together with the standard tone, it is probable that the constant error for an experiment lasting over some time is due to the ear. The mean variation, however, is a continual change from one side to the other, in which there is no time for any influence from the ear. The mean variation therefore indicates the uncertainty of the control over the tension of the vocal cords; it is an error of execution.

Similar results were obtained for higher tones, the errors bearing about the same proportion to the number of vibrations of the tone itself as in the case mentioned. It can be concluded that a good voice will make an error of about 0.4% of the number of vibrations in the tone. For a register extending from F with 88 to d' with 297 vibrations, this allows about 40 distinctly separated tones that can be given with the voice. This interval contains 22 half-tones of the scale; the voice could therefore hardly keep separate, with perfect satisfaction, intervals of quarter-tone. It is remarkable in this connection that the Oriental nations can sing in quarter tones.

[1] The apparatus of Hensen (Du Bois-Reymond's "Archiv f. Physiol.," 1879, 155) for demonstrating the accuracy of the voice is described with illustrations in Scripture, "Thinking, Feeling, Doing," 77, Meadville, 1895.

The number of tones we can hear is limited; if we run upward or downward indefinitely along the scale we finally arrive at a point where we no longer hear tones.

To determine the upper limit of pitch we can most

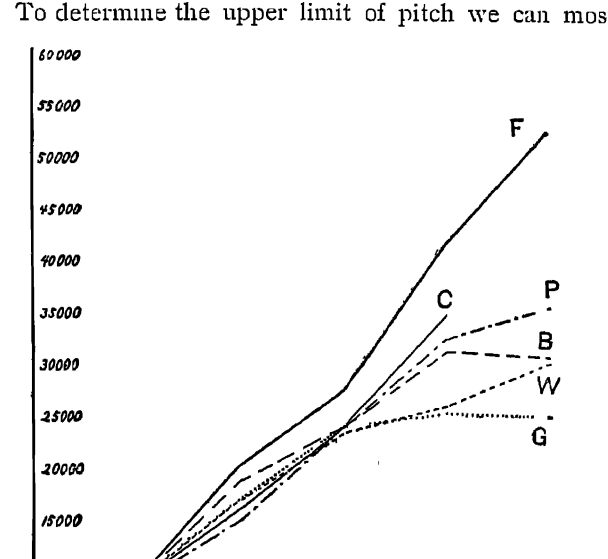


Fig. 77. DEPENDENCE OF THE UPPER LIMIT OF PITCH ON INTENSITY.

conveniently use the Galton whistle.[1] This is a closed labial pipe with an end adjusted by a micrometer screw. As the end is pushed inward the pipe becomes shorter and the tone rises in pitch. The number of

[1] Galton, *Whistles for the Audibility of Shrill Notes*, "Inquiries into Human Faculty," 38, New York, 1883.

vibrations can be calculated from the reading of the screw. As the tone rises there is finally a point at which it disappears for the person listening; this is the upper limit of pitch, or the highest audible tone.

The upper limit of pitch depends upon the intensity of the tone.[1] To determine this relation the whistle is sounded by a blast whose itensity can be regulated. This can be done in the following way. The blast from a rotary-fan blower is carried by a rubber hose to a distant room. The hose ends in a rubber tube having a stopcock and dividing into two branches. The adjustment of the stopcock regulates the force of the blast; this force is indicated by a manometer at the end of one of the branches. The other branch of the tube ends in the Galton whistle. In this manner a constant blast of air can be maintained, and its intensity can be varied at will.

The results of experiments on six subjects are shown in Fig. 77.

It will be seen that for most intensities the upper limit of pitch varies almost proportionately with the intensity; the deviation for the greatest intensity is probably due to the painful character of the tone.

The upper limit of intensity descends with advancing age.[2] This dependence of the upper limit on age was remarked by Galton.

"On testing different persons, I found there was a remarkable falling off in the power of hearing high notes as age advanced. The persons themselves were quite unconscious of their deficiency so long as their sense of hearing low notes remained unimpaired. It is an only too amusing experiment to test a party of persons of

[1] Scripture and Smith, *Experiments on the Highest Audible Tone*, "Stud. Yale Psych. Lab.," 1894, ii. 105.

[2] Zwaardemaker, *Der Umfang des Gehörs in den verschiedenen Lebensjahren*, "Zt. f. Psych. u. Phys. d. Sinn.," 1894, vii. 10.

various ages, including some rather elderly and self-satisfied personages. They are indignant at being thought deficient in the power of hearing, yet the experiment quickly shows that they are absolutely deaf to shrill notes which the younger persons hear acutely, and they commonly betray much dislike to the discovery." [1]

The results of experiments on animals are remarkable. Galton relates :—

"I have gone through the whole of the Zoological Gardens, using an apparatus arranged for the purpose. It consists of one of my little whistles at the end of a walking-stick, that is, in reality, a long tube; it has a bit of india-rubber pipe under the handle, a sudden squeeze upon which forces a little air into the whistle and causes it to sound. I hold it as near as is safe to the ears of the animals, and when they are quite accustomed to its presence and heedless of it, I make it sound; then if they prick their ears it shows that they hear the whistle; if they do not, it is probably inaudible to them. Still, it is very possible that in some cases they hear but do not heed the sound. Of all creatures, I have found none superior to cats in the power of hearing shrill sounds, it is perfectly remarkable what a faculty they have in this way. Cats, of course, have to deal in the dark with mice, and to find them out by their squealing. Many people cannot hear the shrill squeal of a mouse. Some time ago, singing mice were exhibited in London, and of the people who went to hear them, some could hear nothing, whilst others could hear a little, and others again could hear much. Cats are differentiated by natural selection until they have a power of hearing all the high notes made by mice and other little creatures that they have to catch. A cat that is at a very considerable distance, can be made to turn its ear round by sounding a note that is too shrill to be audible by almost any human ear. Small dogs also hear very shrill notes, but large ones do not. I have walked through the streets of a town with an instrument like that which I used in the Zoological Gardens, and made nearly all the little dogs turn round, but not the large ones. At Berne, where there appear to be more large dogs lying idly about the streets than in any other town in Europe, I have tried the whistle for hours together, on a great many large dogs, but could not find one that heard it. Ponies are sometimes able to hear very high notes. I once frightened a pony with one of these whistles in the middle of a large field. My attempts on insect-hearing have been failures." [2]

[1] Galton, as before. [2] Ibid.

There is likewise a lower limit of pitch which can be found by using enormous tuning forks, or slowly vibrating reeds. At this lower limit the tone breaks up into a series of low puffs. This lower limit is generally found somewhere around twelve complete vibrations.

Turning to the property of intensity[1] we first look for a means of measuring the intensity of tones. This is found in the Wien resonator.[2] This consists of a hollow brass sphere. On one side there is an opening of a definite diameter; on the other, a thin metal plate, the top of a capsule from an aneroid barometer, forms a portion of the surface of the sphere. This resonator answers to a tone of a certain pitch. The vibration arriving at the opening sets the spherical mass of air contained in the resonator into strong vibration; this produces a vibration of the thin plate. The stronger the tone the greater will be the vibration of the plate. To measure the extent of the plate's vibration, a minute mirror is arranged to move with it in such a way that it deflects a ray of light. The amount of this deflection is read off in a galvanometer telescope. By careful experiments the relation is determined between the amount of deflection and the amount of energy contained in the sound vibrations at the mouth of the resonator.[3] The sound used by Wien was a tone from a telephone

[1] A musical notation for expressing steps of intensity in addition to the usual factors of pitch and duration was first proposed by Scripture, *Notation for Intensity*, "Am. Jour. Psych.," 1892, iv. 580. It was further developed in "Thinking, Feeling, Doing," 148–152, Meadville, 1895. The notation involves a system of changes in the heads of the notes, and does not interfere with the ordinary notation.

[2] Wien, "Ueber die Messung der Tonstärke," Diss., Berlin, 1888. Ibid., *Ueber die Messung der Tonstärke*, "Annalen d. Physik u. Chemie," 1889, N. F. xxxvi. 834.

[3] The resonators made for the Yale Laboratory have a special tuning adjustment designed by Prof. Wien.

produced by electrical interruptions from a vibrating tuning fork.

With this apparatus the threshold of intensity or the faintest audible tone can be found. For several persons tested by Wien the result showed a fair agreement with his own ear. The intensity of the vibrations was

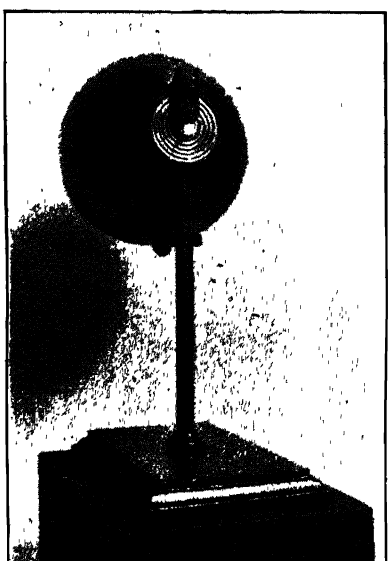


Fig. 78. TONE MEASURER.

0.068 $\mu\mu$ mg.[1] which means that the energy of the air vibrations for this faintest tone was equal to the energy represented in a weight of 1 mg. falling through a distance of 0.068 $\mu\mu$. As the tympanum of the ear has a surface of about 33 square millimetres, the total energy expended in setting it in vibration was 2.2 $\mu\mu$ mg. This

[1] $\mu\mu$ = millionth part of a millimetre ; mg. = milligramme.

amount of energy would be just sufficient to raise 5.1×10^{-12} mg. of water through 1° C.

Wien also investigated the just perceptible difference for various intensities. If the intensity of the tone be indicated by R and the just perceptible difference be indicated by ΔR, Wien's results for various intensities agree approximately with Weber's law of proportionality $\frac{\Delta R}{R}$; that is to say, the just perceptible difference is proportional to the intensity of the tone.

As the Wien resonator has not come into general use, experiments on the intensity of sound are made with purely arbitrary scales. I will select two problems thus investigated: first, the threshold of hallucination, and second, the threshold of sensation during sleep.

In measuring hallucinations of sound,[1] the person experimented upon was placed in a quiet room and was told that when a telegraph sounder gave a signal, he should listen for a very faint tone which would be slowly increased in intensity. As soon as he heard it, he was to press a telegraph key. The experimenter in a distant room had a means of producing a tone of any intensity in the quiet room. The apparatus for producing the tone consisted in an electric fork, interrupting the primary circuit of an inductorium in the experiment room, and a telephone in the quiet room (unknown to the subject), which was in connection with the secondary coil of the inductorium. The intensity of the tone depended on the distance between the two coils of the inductorium.

In the first few experiments a tone would actually be produced every time the sounder gave the signal, but after that the tone was not necessary. It was sufficient

[1] Seashore, *Measurements of Illusions and Hallucinations in Normal Life*, "Stud. Yale Psych. Lab.," 1895, iii. 49.

to give the signal on the sounder in order to produce a pure hallucination.

The persons experimented on did not know they were deceived, and said that all tones were of the same intensity. The real tone could be measured in its intensity, and since the hallucination was of the same intensity it was also indirectly measured.

It is to be clearly understood that the persons experimented upon were perfectly sane and normal. They were friends or students, generally in total ignorance of the subject, who supposed themselves to be undergoing some tests for sensation.[1]

Another illustration of the application of psychological methods is to be seen in the investigations on sleep. A prominent characteristic of sleep, as of various other conditions of consciousness when compared with the condition of maximum consciousness, is the rise of the thresholds in the various senses. The law according to which the threshold for sound is changed with the progress of sleep has been investigated by Kohlschütter, Mönninghoff and Piesbergen, and Michelson.[2]

In Michelson's experiments a measurable sound was produced by the falling of brass balls at various heights on an open board. The subject of the experiment retired to rest not knowing whether any experiment was to be made or not. The experimenter in another room manipulated the apparatus so that balls were dropped from successively greater heights until the sound was loud enough to awaken the subject. This sound can be con-

[1] The principle of the method for measuring hallucinations was first stated by Scripture and Seashore, *On the Measurement of Hallucinations*, "Science," 1893, xxii. 353. A further application of the method to the measurement of the intensity of an imagination is described in Appendix V.

[2] Michelson, "Untersuchungen über die Tiefe des Schlafes," Diss., Dorpat, 1891.

sidered as the just perceptible sound in the given condition of sleep. The experiments were made at different

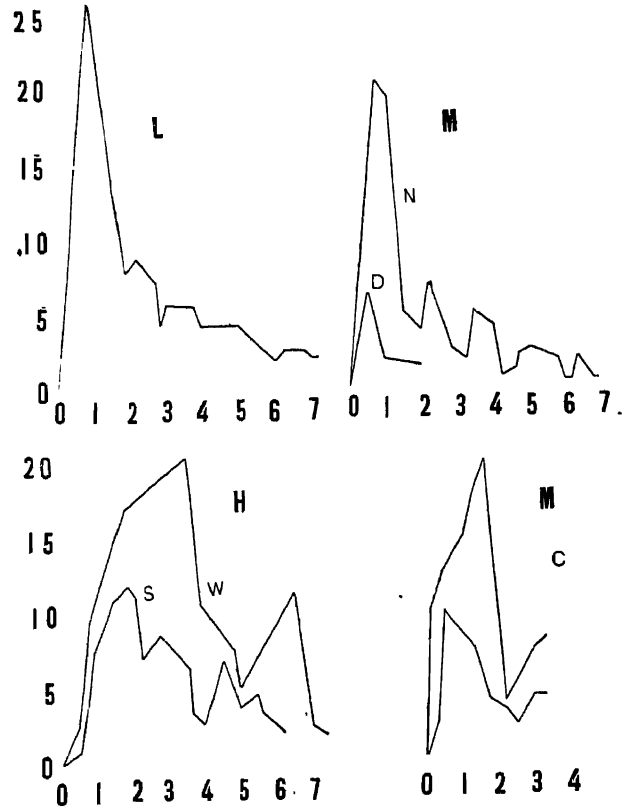


Fig. 79. CURVES OF SLEEP; L, M, H, THREE SUBJECTS; N, NIGHT SLEEP; D, DAY SLEEP; S, SUMMER SLEEP; W, WINTER SLEEP; C, SLEEP FROM NARCOTICS.

intervals of time after falling asleep, not more than two, however, being made in any one night. The general course of the threshold is represented in Fig. 79. With

normal individuals the sleep as measured by the height of the threshold, is rather light during the first fifteen or twenty minutes after falling asleep, but thereafter rapidly becomes deeper, reaching its maximum at ¾ to 1½ hours.[1] In order to wake the person at the moment of deepest sleep, it was found necessary to let a brass ball of nearly ½ lb. weight fall through a distance of 1 metre. After this point the depth of the sleep decreases with considerable rapidity and reaches its first minimum at about the third hour. The further course of the sleep follows, with considerable regularity, an alternation between greater and less depth, the general average depth being steadily less. Day sleep is lighter, but follows the same course as night sleep (D, N in Fig. 79). Winter sleep is deeper than summer sleep (W, S). Narcotics produce strong but short sleep (C). Each person has his own peculiar curve of sleep (L, M, H), but all agree in certain general characteristics.

[1] In Fig. 79 the numbers on the horizontal lines give the hours after falling asleep. The numbers on the vertical lines give the energy of the falling ball in thousands of gramme-centimetres, *i.e.*, the weight of the ball multiplied by the height of fall. Although it cannot be said that the intensity of the sound was proportional to the energy of the falling ball, yet the scale can serve as a fair approximation to a scale of sound-intensities

CHAPTER XXIV.

COLOUR.

ONE of the forms of energy which we perceive is that of colour. Under the term colour all our sensations of light are included. Our investigation, however, must start with the colours actually given us in nature.

In the first place, we will define the spectrum colours by the wave-lengths of the physical vibrations corresponding to them. With the spectrum spread out as a band on the wall, we can tell by the Fraunhofer lines the wave-length for any particular colour. Such a method of defining colours is indicated in Fig. 80.

By increasing and then diminishing the intensity of the light furnishing the spectrum we can cause these colours to pass from the fullest intensity attainable down to perfect blackness. For the present, however, we will disregard the matter of intensity and will confine our account to colours and their combinations of any moderate intensity.

Even then, can we possibly bring the infinity of colours in nature into any system?

In the first place, we can conceive the mixture of any two colours to be represented by points along a straight line. Thus, if a colour R (red) be mixed in various proportions with another B (blue), the results will be represented by points in the line R————————B.

A colour composed of spectrum colours is defined by the relative proportions of the components. Red of a certain wavelength 656.2 $\mu\mu$ mixed in certain proportions with a greenish blue of 492.1 $\mu\mu$ produced white for one subject; in other proportions it produced whitish reds and whitish greens. In our colour system this particular red and this particular green must lie in a straight line with white, thus R——W——G. In a similar manner any other colour must lie on a line drawn between its two spectrum components. Each colour can have only one place. White, for example, can be produced by many pairs of colours; such pairs are called complementary colours. All complementary colours must thus lie at the ends of lines intersecting at a point indicating white. Any other compound colour might be used for this purpose instead of white. As a result of the condition that a colour can occupy only one place, the spectrum colours and their combinations form for each person a definitely united system, the geometric form of which is shown in Fig 81. The colours of such a system cannot be compounded of two elements used in various proportions. We can, however, suppose them to be compounded of three elements. Thes

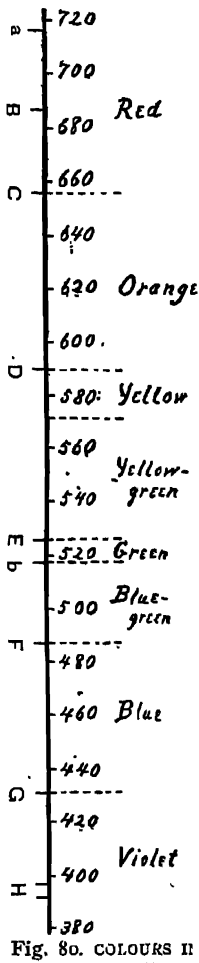


Fig. 80. COLOURS IN THE SPECTRUM.

three colours would be indicated by the corners of a triangle whose area would inclose all possible colours. The spectrum line of Fig. 80 is retained, but is bent around to a curve.

It is at once evident that the three fundamental colours cannot be spectrum colours. No colour triangle can be drawn such that its corner colours shall lie in the curve of the spectrum colours and yet include the other colours. The closest that we can make any triangle conform to the spectrum curve is indicated by

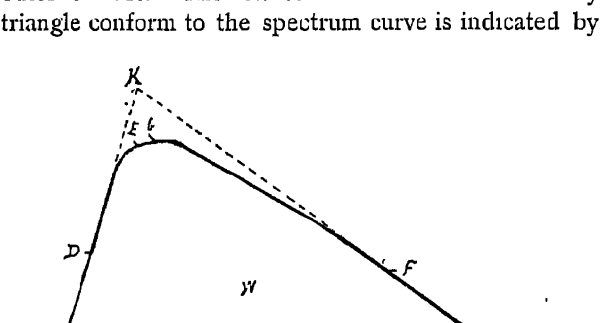


Fig. 81. THE COLOUR SYSTEM ON THE SIMPLEST SUPPOSITION.

the triangle BKV, whereby the violet of the spectrum is supposed to have been rendered slightly whitish by the fluorescence of the retina. If we assume that this triangle represents our mental colour system, our three fundamental colours are spectrum red, a yellowish-green that is not so whitish as the spectrum colour and a violet that is a trifle deeper than the spectrum violet. It is to be noticed that the colours of the spectrum and their combinations do not comprise all the colours we must be able to see. This triangle indicates that the

yellows and greens are more or less whitish as compared with the other colours. There is no necessity, however, for making our colour triangle as close to the spectrum line as possible ; we are at liberty to draw it wherever we find reason to place it. We shall now consider some reasons that give it a definite place.

We have treated the colour system as though it were valid for all persons. This is not the fact. The system we have described is simply typical of the great majority of persons. There are other colour systems ; and, moreover, even individuals of the majority show slight differences.

The colour system of an individual is determined by establishing for him equations between combinations of colours. To illustrate how this is done I prefer to describe one of the latest and best methods rather than the older and less accurate ones, although the latter are the universal ones for pedagogical and practical purposes.

The Helmholtz spectrophotometer for mixing colours [1] is so arranged that the subject sees two coloured surfaces side by side. Each of these surfaces is illuminated by combinations of spectrum colours. The differences in hue are measured by the differences in wave-length and the intensities are considered as proportional to the quantities of physical light.

The simplest form of "colour equation" is found for persons who can match any colour by merely varying the intensity of one colour. One of the colour-surfaces is taken from some place in the spectrum—say the middle, or what to us is green—and remains unchanged except for variations in intensity. The other surface is illumi-

[1] König and Dieterici, *Die Grundempfindungen und ihre Intensitätsverteilung im Spektrum*, "Zt. f. Psychol. u. Phys. d. Sinn," 1892 iv. 243. Helmholtz, "Physiologische Optik," 355, 2 Aufl.

nated in succession from the various parts of the spectrum with light of a constant intensity. By simply increasing or decreasing the intensity of the first surface, the subject can make it appear exactly like any colour that may be thrown on the other surface. The results of such a series of measurements[1] are shown in Fig. 82, where the horizontal line represents the spectrum colours laid off according to wave-lengths, and the curved line H shows the relative intensities of the resulting sensations from different wave-lengths. Thus, the spectrum light of

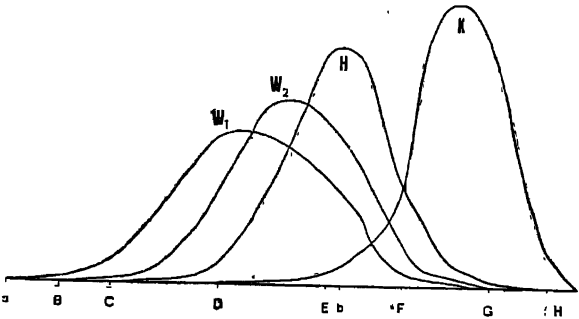


Fig. 82. PROPORTIONS OF THE ELEMENTARY COLOURS IN THE SPECTRUM, FOR MONOCHROMATS AND DICHROMATS.

540 $\mu\mu$ can be matched by the light of 570 $\mu\mu$ by making the physical intensity of the latter 1½ times stronger.

Such persons see the whole world in shades of one colour; as far as "colour" is concerned there is no more meaning in it than there is to normal individuals in a photograph or an engraving. These persons are "monochromatic" in the same sense as a photographic plate is monochromatic; all the variations of the world of colour are reduced to a system of intensities of one colour. The arrangement of intensities is, however, not the same

[1] König and Dieterici, as before.

as with the photographic plate. For the ordinary plate the blues are high lights, the reds are nearly the same as black, and the greens are quite dark. It is true that the so-called orthochromatic plates change the result to some extent by making the greens and reds brighter, so that the picture more nearly represents the relative intensities as they appear to the normal eye. Even this, however, does not correctly represent the appearance to the monochromat. The curve H in Fig. 82 shows that, for the monochromat from which it was obtained, the brightest colour corresponds to our green, that the blues and yellows were dark, and that the extreme reds and violets were absolutely black. This subject of König's measurements stated that for him the usual representations of landscapes by engravings never gave a proper reproduction of the relations of brightness; for him the fields and forests were almost always the brightest objects in a landscape, whereas in the drawings produced by other persons they were dark. It would be interesting to learn what colour of our system corresponds to the one colour of the monochromats; there are, however, at present no data for settling this point.

For another class of persons any colour of the spectrum shown on one of the surfaces of the spectrophotometer can be matched by a combination of particular intensities of two other spectrum colours. For these "dichromats" there are two sharply limited regions at the ends of the spectrum within which there are no changes of hue, but merely of intensity. All the other parts of the spectrum, the "middle region," can be produced by mixtures of the two end regions. The colours of the two end regions can be considered as the elementary colours; they are most conveniently called the "warm" colour for the red end, and the "cold" colour for the violet end.

Using colours from the two end regions König and Dieterici[1] have determined the proportions necessary to match any other colour in the spectrum.

The proportions of the cold colour were nearly, but not quite, the same for all four subjects (Fig. 82, K). The proportions of the warm colour were quite different; two of the subjects agreed closely in following a certain law, and the two others agreed fairly in following quite a different law (Fig. 82, W_1 and W_2). It was quite evident that the subjects belonged to two different forms of dichromasy.

The whole world of colour thus appears to a dichromat as a mixture of two colours, somewhat in the same way as a landscape would appear to us if painted in red and violet or in green and violet.

The most common class of people is that of the "trichromats"; these include almost all women and about 96% of the men. When one of the surfaces in the colour-mixer is illuminated by a spectrum colour, it is not often possible to match that colour by a mixture of two other colours; for most of the spectrum a third colour must be employed.

For the trichromats the end regions of the spectrum are of a constant hue and differ only in intensity. Just inside of each end region there is an intermediate region in which any colour can be produced by mixtures of the end colour with a colour of the intermediate region. Between these intermediate regions lies the middle region, which requires the presence of some third colour in addition to colours from the end regions. These regions extend, with very small individual differences, over the spectrum as follows[2]:—

[1] As before, p. 259.
[2] König and Dieterici, as before, p. 283.

Warm end region, from extreme red to 655 $\mu\mu$.
Warm intermediate region, from 655 $\mu\mu$ to 630 $\mu\mu$.
Middle region, from 630 $\mu\mu$ to 475 $\mu\mu$.
Cold intermediate region, from 475 $\mu\mu$ to 430 $\mu\mu$.
Cold end region, from 430 $\mu\mu$ to extreme violet.

To establish the colour equations for a trichromatic system it is necessary to determine quantitatively the various complementary colours. This was done by König and Dieterici for over seventy persons, finding that all but three agreed closely on one system of results,

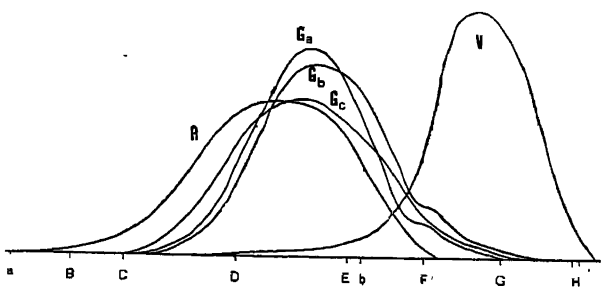


Fig. 83. PROPORTIONS OF THE ELEMENTARY COLOURS IN THE SPECTRUM, FOR TRICHROMATS.

while these three agreed on a different system. The former are justifiably called the "normal trichromats," and the others the "abnormal trichromats."

Measurements on two normal and one abnormal trichromat gave results as indicated in Fig. 83.

The proportions of red and violet corresponded closely for all three; the results are indicated by the curves R and V. The proportions of green differed considerably for the two normal trichomats, G_a and G_b and still more for the abnormal trichromat G_c.

The colour curves which have been used up to this point represent the proportions of elementary colours

which are actually required to produce the spectrum colours and their combinations.

From the equations for elementary and compound colours of the spectrum we can draw conclusions concerning the psychological, or fundamental, colours.

In the first place, we must conclude that the number of fundamental colours must be the same as the number of elementary colours.[1]

For the trichromats the fundamental colours will be derivable from various proportions of the three elementary colours. Thus, for the normal trichromats, the fundamental colours are derived from the elementary colours R, G, and V, by—

$$\mathfrak{R} = a' \,.\, R + b' \,.\, G + c' \,.\, V$$
$$\mathfrak{G} = a'' \,.\, R + b'' \,.\, G + c'' \,.\, V$$
$$\mathfrak{B} = a''' \,.\, R + b''' \,.\, G + c''' \,.\, V$$

where the co-efficients a, b, and c may have any values, including o. Likewise for the abnormal trichromats the fundamental colours are derived by—

$$\mathfrak{R}' = a_1' \,.\, R' + b_1' \,.\, G' + c_1' \,.\, V'$$
$$\mathfrak{G}' = a_1'' \,.\, R' + b_1'' \,.\, G' + c_1'' \,.\, V'$$
$$\mathfrak{B}' = a_1''' \,.\, R' + b_1''' \,.\, G' + c_1''' \,.\, V'$$

For the dichromats we have for the first class—

$$\mathfrak{W}_1 = \alpha_1' \,.\, W_1 + \beta_1' \,.\, K$$
$$\mathfrak{K}_1 = \alpha_2'' \,.\, W_1 + \beta_1'' \,.\, K$$

And for the second—

$$\mathfrak{W}_2 = \alpha_2' \,.\, W_2 + \beta_2 \,.\, K$$
$$\mathfrak{K}_2 = \alpha_2'' \,.\, W_2 + \beta_2'' \,.\, K$$

[1] König and Dieterici, as before, p. 324.

For the monochromats the fundamental colour will be—

$$\mathfrak{Y} = a\ \mathrm{H},$$

where H is the spectrum colour chosen as elementary.

The relations between these systems are to be determined by experiment and by computation.[1] If such relations exist, the colour equations established experimentally for a more complex colour system, must seem correct for a less complex system. Then, by computation, values can be found for a, b, c, α, β, in the equations

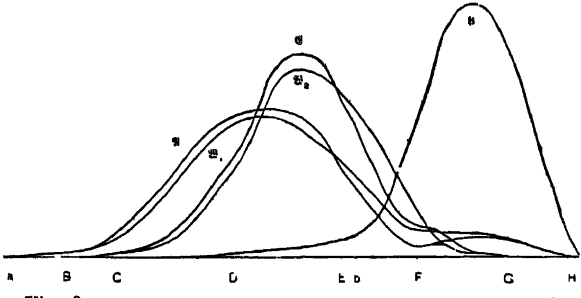


Fig. 84. PROPORTIONS OF THE FUNDAMENTAL SENSATIONS IN THE SPECTRUM.

just given, such that the curves thereby deduced for $\mathfrak{Y}$, $\mathfrak{M}_1$, $\mathfrak{K}_1$, $\mathfrak{M}_2$, $\mathfrak{K}_2$, $\mathfrak{R}'$, $\mathfrak{G}'$, $\mathfrak{B}'$, shall coincide with some or all of those deduced for $\mathfrak{R}$, $\mathfrak{G}$, $\mathfrak{B}$.

For the normal trichromats the proportions of the fundamental colour sensations in the spectrum colours are shown in Fig. 84, $\mathfrak{R}$, $\mathfrak{G}$, and $\mathfrak{B}$. Taking the spectrum curve as shown in Fig. 81, the fundamental colours would be at the corners of the triangle in Fig. 85.

The fundamental red would be a deep carmine,

[1] König and Dieterici, as before, p. 326.

the red of the spectrum being somewhat whitish and yellowish. The fundamental green is far greener than spectral green, and the fundamental blue corresponds nearly to indigo blue.

For the abnormal trichromats the 𝔊′ colour differs from the 𝔊 of the normal trichromats, but its exact nature has not been determined.

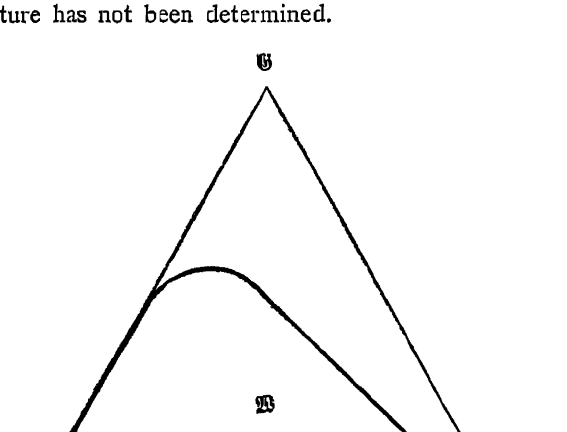


Fig. 85. THE PSYCHOPHYSICAL COLOUR TRIANGLE.

For the dichromats there is a definite relation to the fundamental colours of the trichromats. The warm fundamental colour of the first class $\mathfrak{W}_1$ coincides very closely (Fig. 84) with the red fundamental colour $\mathfrak{R}$; and that of the second class, $\mathfrak{W}_2$, with the 𝔊 of the trichromats; while for both classes the cold colour coincides with the fundamental 𝔅.

It need not be supposed, however, that the dichromats differ from the trichromats merely in the lack of one

fundamental colour, 𝔊 or 𝔎 ; they are not "green blind" and "red blind" as usually stated. The conclusion seems justified[1] that the 𝔎 and 𝔊 sensations are both present, but that their curves coincide with the 𝔎 curve of the trichromat in one class, and with the 𝔊 curve in the other class. The result is a yellow. All the colours of the spectrum are therefore produced by combinations of yellow and blue ; the two classes differ in the proportions.

For the congenital monochromats, so far as investigated, the fundamental colour does not coincide with any one of the fundamental colours of the other systems. The colour equations established by other persons are not considered as correct by the monochromats. Congenital monochromasy cannot be considered to have arisen from trichromasy by the loss of two of its fundamental colours, although such a form of pathological monochromasy may perhaps occur.[2] The colour triangle of the trichromats, which shrinks to a line for the dichromats, becomes a mere point for the monochromats ; but the point around which it shrinks is left undetermined.

A particular case of monochromasy has been found in which both the red and the blue fundamental colour coincided in general with the green curve. This subject was a monochromat because all three sensations were constantly present in the same relative intensities

[1] König and Dieterici, as before, p 344 ; Helmholtz, "Physiologische Optik," 458, 2 Aufl. ; König, *Eine bisher noch nicht beachtete Form angeborener Farbenblindheit*, "Zt. f. Psych. u. Physiol. d. Sinn.," 1894, vii. 71.

[2] Konig, *Ueber den Helligkeitswert der Spektralfarben*, "Beiträge z. Psychologie u. Physiologie d. Sinnesorgane, Helmholtz gewidmet," 374, Hamburg, 1891.

[3] König, *Eine bisher noch nicht beachtete Form angeborener Farbenblindheit*, "Zt. f. Psych. u. Phys. d. Sinn.," 1894, vii. 161.

presumably the resulting colour was white and its shades.

The colour triangle that we have been using to represent the psychological laws of colour has not been laid off with psychological units, but with arbitrarily assumed physical units of wave length and intensity. It consequently gives a distorted picture of our colour system.

A method of determining the psychological colour triangle and the three fundamental colours has been proposed by Helmholtz.[1] The unit of measurement for colours is the size of the just perceptible difference. Suppose we start from any arbitrary colour, say a certain pink. As we change this colour toward any other colour, we find a certain just perceptible difference, which varies with the direction in which the pink is changed. In this way we can map out the whole colour surface within the spectral curve. As far as this has been done, the results agree with Helmholtz's extension of Fechner's law. This law of Helmholtz expresses the fact that a difference in a sensation resulting from the union of three components bears a relation to differences in the components in the following way: $dE^2 = dE_1^2 + dE_2^2 + dE_3^2$ where E_1, E_2, and E_3 are the components and E the resultant sensation. Assuming Fechner's law as valid for each component—as experiments justify us in doing—we have as a final expression

$$dE = K \sqrt{\left(\frac{dx}{x}\right)^2 + \left(\frac{dy}{y}\right)^2 + \left(\frac{dz}{z}\right)^2}$$

where x, y, and z denote the three colours used for producing the mixture E, and K is a constant.

Helmholtz's development of this formula with the practical verification on the basis of experiments by

[1] Helmholtz, "Physiologische Optik," 439, 2 Aufl.

König, Dieterici, and Brodhun, render it possible to arrange the spectrum colours according to the unit of distinctness, which is the reciprocal of the least perceptible difference. The result is indicated in Fig. 86.

In the psychological triangle the colour values of the three fundamental colours are assumed to be equal;

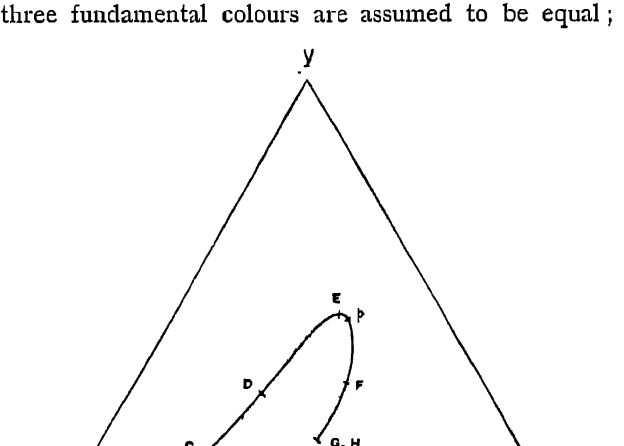


Fig. 85. THE PSYCHOLOGICAL COLOUR TRIANGLE.

they are therefore placed at the angles of an equilateral triangle, with white in the middle. The spectrum colours occupy the position indicated by the curved line.

The spectrum colours are thus all of them combinations of the three fundamentals, and are to be considered as whitish colours. The red fundamental colour corresponds to a deeper carmine red than can be produced in nature; the spectrum red is a whitish, yellowish

modification of this colour. The green fundamental colour corresponds to a highly saturated yellowish green of the spectrum (between 540 $\mu\mu$ and 560 $\mu\mu$), or the colour of vegetation. The blue fundamental colour corresponds to a highly saturated ultramarine.

The colour triangle is not adequate to represent our whole colour system. The area included by the spectrum curve and the line joining red and violet will represent all the colours of nature and their combinations, for any one system of intensities. But as we darken the spectrum colours, the whitish colours and white itself, we get a steady series of changes in the system that cannot be represented unless we have recourse to a solid figure.

Suppose all the colours of nature, the reds, greens, browns, pinks, greys, &c., to lie before us in the form of little cubes. If we attempt to arrange them according to similarity, we find it impossible to do so on a flat surface. Beginning, say, with green, we find colours that lead us by just perceptible steps over toward blue, and from that to violet on one side, and over toward yellow, and from that toward red on the other side. This series of colours we might arrange in a straight line. We also find colours that lead us gradually from each colour in our straight line over toward white. We might arrange these colours in lines on one side, at right angles to the first line. Again, there are colours leading from the original ones steadily toward black. These can be arranged on the other side of the first line. But the great majority of colours is still left. These are the more or less greyish colours that lead by continuous steps from one of the whitish colours to one of the blackish ones. There is no place for these colours on the flat surface. Our colour system, to use Riemann's expression, has three dimensions ; that

is, any given colour can be determined by reference to three (not less) independent quantities.

Like all other quantities of three dimensions, the colour system is most conveniently represented as a geometrical figure in our space of three dimensions.

All the colours, as we have seen, can be represented as formed by the combination of various proportions of three fundamental colours.[1]

A complete representation of our colour-system can be made by supposing the three fundamental colours to be laid off with proportional units on the axes of X, Y, and Z in a system of three co-ordinates.[2] Any particular colour i will be then completely defined by the equation $i = x + y + z$. This is illustrated in Figs. 87 and 88.[3]

In this figure all possible colours are supposed to have places. Red of steadily increasing intensity is represented by the X axis, green by the Y axis, and blue by the Z axis. Black, or no colour, is at the origin of co-ordinates. The greys extend along the line from the origin outward at equal distances from the three axes. The brightest obtainable grey is called white.

The greatest possible values for R, G, B, W, and the other colours remain unknown. The light of the mid-day sun reaching the earth's surface through a clear

[1] Grassmann, "Die Ausdehnungslehre von 1844," 42, Leipzig, 1878. Helmholtz, *Zählen und Messen erkenntnisstheoretisch betrachtet*, "Philosophische Aufsätze, Eduard Zeller gewidmet," Leipzig, 1887.

[2] Lambert, "Farbenpyramide," Augsburg, 1772. Helmholtz, "Physiologische Optik.," 336, Hamburg and Leipzig, 2 Aufl.

[3] These figures and all other double ones can be seen as solid objects by means of the stereoscope. With the American model of the stereoscope the book is simply held where the picture is usually placed. It is still more convenient to cut off the front stick. With the German model the front carrying the prisms is simply bent upward. Persons unaccustomed to the stereoscope may have to look steadily at the figure for a few seconds before the attention becomes fixed on the figure instead of the paper.

sky is assumed as a value for white, and the three

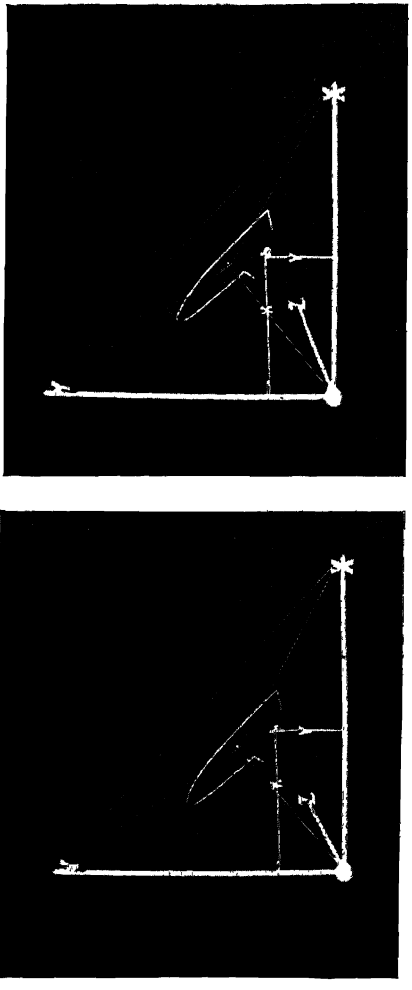


Fig. 87. SIDE VIEW OF THE COLOUR PYRAMID

fundamental colours are supposed to be present in

equal proportions. This cuts off the system of co-ordi-

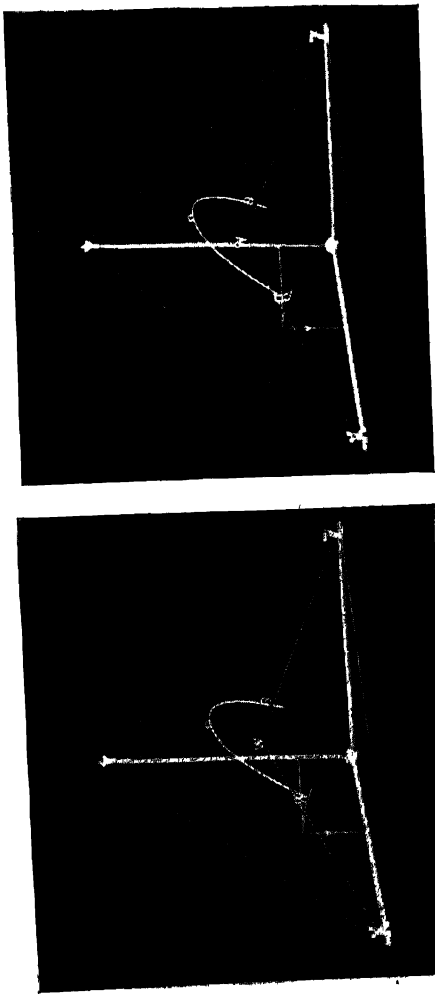


Fig. 88. END VIEW OF THE COLOUR PYRAMID SHOWING THE COLOUR TRIANGLE AND SPECTRUM CURVE.

nates by a plan at right angles to the grey line. The

face of this plane gives us the colour triangle for the maximum intensity. The exact position of the spectrum colours within this maximum colour triangle is still undetermined. The colour triangles we have considered all refer to moderate intensities. It is known that with high intensities the spectrum colours all tend to pass into a blue, and that with low intensities the result is a kind of monochromasy. In Figs. 87 and 88 I have supposed the system of co-ordinates to be cut off at a moderate intensity, so that the section represents the usual colour triangle.

The fact that all the colours of nature may be produced by combinations of three fundamental colours can be beautifully demonstrated by the tricolour lantern with appropriate slides.

The tricolour lantern was originally the invention of Duhauron. The technical difficulties, however, in the way of manipulation, have only lately been completely overcome by the manufacturers. This lantern is a very compact triunial (Fig. 89). In front of the condensers transparent media—glass or gelatine—are placed; red for the lower, green for the middle, and blue for the upper one. The lenses are adjusted to throw the three colours to the same place on the screen.

We have thus three colours at command which we can combine in any desired proportions. These colours are approximately of the wave-lengths 680 $\mu\mu$, 540 $\mu\mu$, and 450 $\mu\mu$. Referring to the spectrum curve of the colour triangle (Fig. 86), we find their positions to be such that the triangle formed by lines joining the three points includes more of the colours of nature than could be included by any other three colours. Since we cannot go beyond the colours of nature, *i.e.*, outside of the spectrum curve, for the three colours, some of the colours must necessarily, suffer, as explained on p. 332.

The violets and purples produced with these three

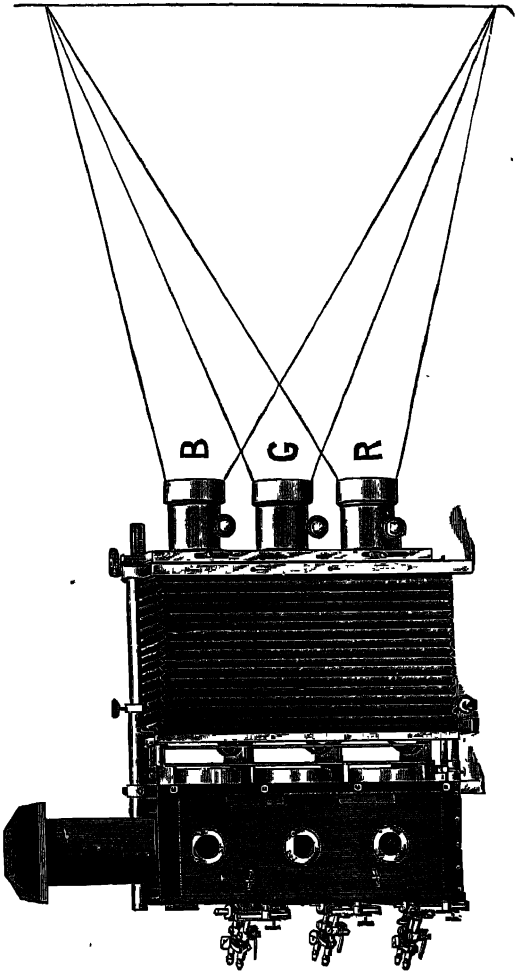


Fig. 89. THE TRICOLOUR LANTERN.

colours have a somewhat whitish appearance.

Having at our command three colours for combination, it becomes possible for us, with the appropriate accessory apparatus, to demonstrate on a large scale all the phenomena of the psychology of colour usually shown by colour wheels, prisms, &c. As these accessories have not yet been completed, I can say no more at present than that everything that can be done with the Maxwell colour discs, which are small and not luminous, can be done with all the brilliancy of the limelight. One great objection to the paper discs is the loss of light, whereby mixtures of colours are all dull and greyish and a white cannot be obtained. Here every new colour introduced into a mixture adds to the light, and a bright white can be produced.

The fact of the composition of the natural colours by mixtures of three fundamentals in varying intensities can be strikingly brought out in the following way. A group of brilliant flowers, say, a stalk of white gladiolus in the middle, one of red on the left, and one of blue on the right, is placed before a grey background. A camera is focused on them, and three successive photographs are taken, one through a red gelatine ray filter, one through green, and one through blue. The result is a set of three negatives, all alike in form, but different in the shading. Three positives are now made on glass; these are also alike in form, but different in shading. Such a series of positives is shown in Fig. 90. The white gladiolus is bright in all three. White light can be analysed to the three components, red, green and blue, and each of the coloured ray filters lets some light through. The red gladiolus on the left is bright only on the red plate, because the blue and green ray filters kept the red rays off the others. The tips of the leaves, however, affected the blue plate, showing that the tips must have been

somewhat purplish. The bluish gladiolus on the right affected all three plates, but mainly the red and blue;

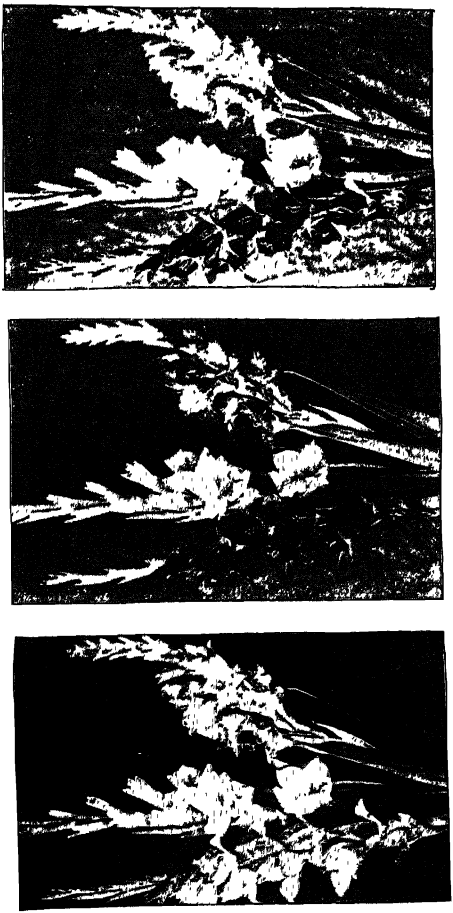


Fig. 90. SET OF SLIDES FOR THE TRICOLOUR LANTERN.

they had, in fact, a purplish tinge with white markings. The green stalk affected mainly the green plate. It

is to be remembered that these three positives are uncoloured, and differ merely in the shadings, as shown in Fig. 90, which was actually made from a set of them.

These positives are mounted accurately as slides in a single frame one above the other. The frame is then inserted into the tricolour lantern with the red slide before the red light, &c. As a result the three pictures are thrown simultaneously on the screen in the three appropriate colours. The result is a picture in all the colours of the original. As we have seen, any given colour can be produced by various proportions of the three fundamentals. Here the various proportions are produced by the shading of the slides. For any given spot on the screen the colour i is produced the shading of that spot in each of the three slides, whereby the red slide permits x units of light to pass, the green y units, and the blue z units.

This method is, of course, applicable to reproductions of landscapes and all other natural objects. The view reproduced on the screen is, curiously enough, more beautiful than the original. This arises from its smallness and its limitation by a definite line, whereby there is an increased concentration of attention to a definite place. The effect resembles the original view as if seen at a moderate distance through a window. The beauty of scenes such as the Bay of Naples, the Jungfrau from Interlaken, &c., are, when produced in this way, beyond description.

The phenomena of colour blindness can also be represented with the tricolour lantern.

The usual theory of colour blindness, according to which the defect arose by the failure of one of the three fundamental colours, can be illustrated by covering up one of the lenses. For red blindness the red lens is covered, and the resulting picture appears in

combination of green and blue ; for green blindness the green lens is covered, and for the hypothetical blue blindness the blue one is covered. To illustrate the newer theory, the blue slide is left unchanged, but two slides are made for red and two for green. For the dichromats of the first class—the red-blue persons—the two slides taken through the red ray filter are placed in the red and green lanterns. Thus, in the case of the gladiolus, the slide R is thrown on the screen in red light from one lantern, and also in green light from the second lantern, while the B slide is thrown in blue as usual. The G slide is not used. The result is a picture in combinations of yellow and blue.

For the other dichromats—the green-blue persons—the G slide is thrown in red light, and again in green light, while the blue remains the same. The R slide is not used. The result is also a picture in combinations of yellow and blue, but each particular combination differs from that in the previous case.

To illustrate monochromasy one lantern alone is used, the colour being left to an arbitrary choice.

PART IV.

SPACE.

CHAPTER XXV.

STANDARDS OF SPACE.

WHEN the two points of a pair of opened dividers are allowed to rest equally on the skin, two sensations of touch are felt at the same time and with the same energy. There is thus co-existence without identity. Our ideas of number are presumably derived from such co-existences among different senses and in the same sense.

Starting with the two points of the dividers felt simultaneously, we can insert a third point between them; then additional points between these, and so on, till the series of points fuses into a continuous line. We have already seen how a sensation of pressure may vary in time and in intensity. We here find still another property, such that two sensations may be alike in time and intensity and still be different. This we call the space property of our pressure-sensations, or tactual space.

Similarly our sensations of light have the two properties of time and intensity, also a third one of hue, and, finally, a fourth one, which we call visual space. For

an odour, however, when we have stated its quality, *e.g.*, rose odour, its intensity and its time of occurrence, we have exhausted its properties. The odours from two roses—different in space for both sight and touch, but present at the same moment—are not separated by the nose; the removal of one rose produces a difference for sight and touch which is not one of intensity, whereas for the nose the result is merely a decreased intensity.

The term space, as here applied, does not imply any similarity of these properties of touch and sight to the mechanical space of physics. In physics space is a form of energy—distance-energy, space-energy—and no more resembles tactual or visual space than ether waves resemble sensations of colour. When one physical distance is said to be twice as great as another, the meaning is that the movement of a material point through the greater distance represents twice as much energy (ergs, grammes of water heated through 1° C., &c). When such exhibitions of energy are seen by us, the distance-energy appears under appropriate conditions, as visual space; when they are felt, it appears as tactual space. There is, however, no resemblance between the physical space and our sensation-spaces.

Furthermore, our sensation-spaces have no resemblance to each other. Our tactual space is a property of our pressure-sensations and our visual space is a property of our visual sensations; there is no more resemblance between them than there is between the intensity of a pressure and the intensity of a light. Physical space-energy corresponds to these two properties in sight and touch, and we are accustomed to associate them together.

The total unlikeness of the properties grouped as intensity is fairly well recognised, owing to the fact that

the intensity in mechanical, chemical, and thermal energy corresponds to intensity in so many different classes of sensation. The total unlikeness of the space-properties has been obscured by the overpowering *rôle* played by our sense of sight,[1] whereby we find it scarce possible to attend to our tactual space without picturing our visual space also. To clearly bring out the difference persons were needed who on a given occasion, accessible to experiment, for the first time attempted to compare the two systems of space-properties. These persons have been found in those born blind (or blinded at a very early age), to whom sight was restored by successful operations. The results show that forms familiar to touch are not recognised by sight when seen for the first time.

In spite of this total difference between sight and touch we find that the quantitative relations of space-energy established for sight hold good in a rough way for touch also. Three nails arranged at equal distances by the eye will be felt by the hand to be at about equal distances. We are thus enabled to establish a standard-space including both.

[1] The fact that time is a group of properties in which various classes of sensations furnish a certain agreement without likeness, seems to have obtained little recognition. A certain property of sight is measured by means (*e.g.*, terrestrial revolutions, clock, forks, &c.), similar to those used for a certain property of sound, pressure, smell, &c., &c. These properties are grouped under the term "time." There are as many different kinds of time as there are groups of sensations. These times are unlike. A time in pressure is a pressure; a time in tone is a tone. If the property of tone which we have called pitch had coincided practically with the time-properties of the other sensations, pitch and duration in tone would have changed places. Possibly some day an intelligent subject will be operated upon so that hearing is acquired for the first time; in such a case the experiment can be devised to ascertain if he finds any coincidence between an interval of sight-time and an interval of tone-time, of tone-intensity or of pitch.

In finding standard time we sought for the greatest amount of agreement among recurring events as we noticed them, and finally ended with adopting the successive passages of a star across a meridian as the standard for practical use (Chap. V.).

Likewise in finding the standards of energy we found a correlation to exist between different kinds of experience, whereby we could establish relations with the form of energy appearing in weights (Chap. XIV.).

In regard to space we can produce lines of fused points as felt on the skin, and as seen by the eye. Some of these lines are bigger than others, but between any two points there is a line that appears smaller than all others between those points. Such lines are "straight" lines. In concrete cases an object producing the sensation of a straight line in one place or for one sense may not produce one of a straight line in another place or for another sense; but proceeding exactly as for time and energy, we can, with the aid of apparatus, establish a standard straight line. This gives one of the standards of direction.

From the standard, or mathematical, straight line all the geometrical standards can be derived, *e.g.*, plane and curved surfaces, area, form, &c. A specially important standard is that of angular direction. Abstracting from concrete cases we can represent a revolution by that of a line around an axis. When this revolution occurs in such a manner that the area around the axis which is covered by the line, is a minimum, we have the circle. The circle is adopted as the standard and unit of angular direction, and is divided into sub-units: degrees, minutes, and seconds.

We have thus the standards of direction; one other standard is required, namely, of distance. The standard of distance is an arbitrary straight line; this is the

distance between two marks on the metre-bar at Paris. A metre with its multiples and sub-units is established by methods of judging agreements as in the case of time.

The metre is the standard for visual space of all kinds. A bar of metal seen by us changes its apparent length as we move or it moves, but every time it comes back to the original position, it seems about as big as before. Suppose we place one bar in a definite position, say horizontally sideways, and then cut off another bar of equal length when the two are laid together. No matter how much we may move one of the bars, the two will match again as soon as brought together. We thus assume the metre under all circumstances as the standard.

For touch the distances felt on the skin are likewise standardised as parts of the metre.

The psychological nature of the standards of space can be illustrated by the consideration that if all space were made $\frac{1}{2}$, $\frac{1}{3}$, ..., $\frac{1}{n}$, or 2, 3, ..., n times as large as it is we would not know the difference. In fact, it might very well be supposed to happen that the whole universe is—over night while we are asleep—suddenly magnified or microfied by some miraculous power. Upon awakening, we should not know the difference by sight or by touch, for our standard metre with its multiples and sub-multiples, and also all objects by which we estimate size, have changed in exactly the same ratio.

One important consequence at once follows from this relativity of standard space : a comparison between the spaces of different individuals is—as far as space goes—perfectly meaningless. Who will undertake to say that the scene he sees, is of the same size to him as to his neighbour, to his dog, to a fly? His own space is a definite affair measured by a standard metre ; so is that of every other space-perceiving organism. But may not the space of a fly be the $\frac{1}{n}$th part of his space? As he

would not know, if his own space were reduced to the $\frac{1}{n}$th the size, he surely would not know the difference if he could see space like a fly.

There is, nevertheless, a possibility of comparing spaces indirectly. This possibility suggested itself after the perusal of Delbœuf's "Mégamicros, ou les effets sensibles d'une reduction proportionelle des dimensions de l'univers."[1] The consideration was suggested by a passage in Laplace's "Exposition du système du monde" (liv. v., ch. v.), in which it is asserted that one of the remarkable properties of the law of attraction (intensity inversely as the square of the distance) is as follows. If the dimensions of all bodies of the universe, their mutual distances and their velocities were to increase or decrease proportionally, they would describe curves exactly similar to those they now describe, with the result that the universe thus reduced successively to the smallest imaginable space would offer always the same appearance to the observers. These appearances are consequently independent of the dimensions of the universe, as in virtue of the law of proportionality of force to velocity they are independent of the motion in space.

The psychological deductions contain a remarkable truth and a remarkable error. The former is that of the complete relativity of space as a phenomenon of vision. The error is pointed out by Delbœuf. I can probably best state it by relating some experiments of Delbœuf's Mégamicros, an inhabitant of the earth suddenly transported to an imaginary planet (similar to Mars) at half the earth's distance from the sun with everything—even himself—reduced to half size. The Martians, as the inhabitants of this planet are called, have established the metre, the hectare, and the litre just as we Terrestrians have done. The Martian

[1] "Bulletins de l'Academie royale de Belgique," 1893, (3) xxv. 667.

metre is, of course, actually only $\frac{1}{2}$ the length of the Terrestrian metre; similarly the hectare is only $\frac{1}{4}$ and the litre is only $\frac{1}{8}$ of ours. But as all things are reduced in proportion, Mégamicros detects no difference. The Martian unit of weight is likewise that of a litre of distilled water, named the kilogram. Its size is $\frac{1}{8}$ of ours, but its weight is only $\frac{1}{16}$, owing to its smaller distance from the centre of gravity. The muscular power of Mégamicros is proportional to the volume of his muscles, or $\frac{1}{8}$ of its former power. Upon lifting a familiar object he finds it astonishingly light. On the earth his pitcher of water weighing two litres and raised through 30 cm. would require a certain effort; on the imaginary Mars, weighing only $\frac{1}{16}$ as much and being moved through $\frac{1}{2}$ the distance, it would require the performance of only $\frac{1}{32}$ as much work whereas the measure of muscular effort (mass × time) would be reduced only to $\frac{1}{8}$. Consequently the pitcher would appear to weigh only $\frac{1}{4}$ as much as expected. Similar experiences would occur for every effort.

Applying the illustration to ourselves, we could not detect an expansion or contraction of the universe by our space perceptions, but we could do so by the results following upon voluntary efforts of given intensities. Our voluntary efforts form a fairly constant standard of judgment, and, knowing no reason for any such change in results, any great or sudden change in the dimensions of space would undoubtedly appear as such to us. Delbœuf draws the conclusion that, since we do not perceive such changes, the real dimensions of the universe are constant.

But we really do perceive such changes in space. On some occasions, *e.g.*, a bright day, or after a stimulant, a mile is a small distance; on other occasions, *e.g.*, at the end of a journey, the road seems long. It is to

be noted that, although we make some correction on account of our knowledge of the changed condition of effort, the change is perceived by us as a change in the dimensions of space. Other circumstances, *e.g.*, railroads, bicycles, &c., that have diminished the effort required for space experience, have actually caused portions of the earth to grow psychologically smaller.

Conditions similar to fatigue can be artificially produced. In my experiences (p. 174) with *Cannabis Indica* there was apparently a weakening of will-power, and a consequent magnification of distance seemed to occur.

In conditions of exhilaration time and space are small affairs. The unusual effects resulting from the muscular exhilaration of the maniac must make the world small and trivial for him.

We have thus a possibility of comparing the relative sizes of our space at different times and for different individuals. For want of any definite knowledge at present concerning the relations we may, for illustration, assume that the spaces are inversely proportional to the feelings of effort. As the standard of effort, we may conveniently take the maximum effort. When the results of the maximum effort are the same for a given individual on different occasions, or for different individuals, the spaces are equal; when the results are greater the spaces are less, and likewise the reverse.

The experimental determination of the law must, of course, limit the problems to particular circumstances, *e.g.*, the subject might be required to exert a maximum pressure on the dynamometer, and also to indicate a distance of one metre on a blank wall extending beyond the field of vision and having absolutely nothing to serve as a standard. We would undoubtedly find some law of relation between the distance indicated as a metre and the changes in the maximum pressure.

CHAPTER XXVI.

BODILY SPACE.

AMONG the many kinds of space—monocular space, binocular space, tactual space, &c.—there is one to which all the others are related. It is the space of "where we are." It might well be called the fundamental space, or, perhaps preferably, the bodily space.

In designating the place of anything there must be a fixed point and a fixed direction to which to relate it. This point and this direction we find in ourselves. When we know where we are, we can tell where other things are; when we have lost ourselves, all localities are in vagueness. It is very important therefore to inquire: Where are we? Some one might answer that we are "here, with head up and feet down"; that would simply be a re-statement of the question, whereas what we wish to know is: What do "up" and "down" mean? What does "here" mean?

Let us start with the question of up and down. We place a person on the tilting-board (Fig. 91), with eyes closed.

When we stand him upright, he notices various sensations of pressure, *e.g.*, from the soles of the feet, from the tension of various parts of the skin, &c.; there are also various sensations from the joints, and finally various

sensations from the sinews, *e.g.*, strong ones from the legs and weaker ones from the arms.

A pointer is put into his hand and he is to keep the pointer vertical. As we tip him gradually backward around the horizontal axis, he keeps the vertical quite correctly for a while, but as he is turned further the vertical appears to turn also ; *i.e.*, he under-estimates the

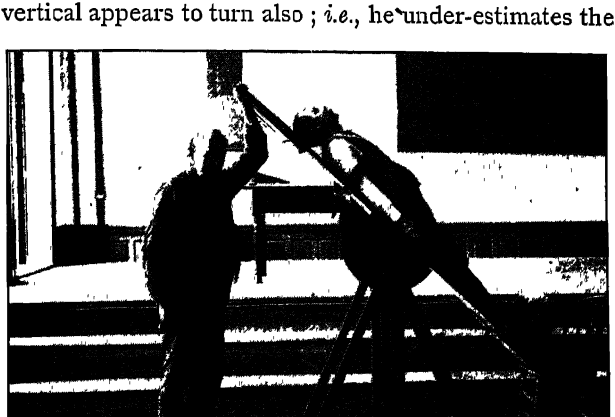


Fig. 91. THE TILTING BOARD.

amount through which he is turned. Similar results probably hold true for tipping in all directions.

The changes in the body-sensations are what he considers to be changes in direction. He can assume any group of them as a standard, say the group he feels when he is standing. This group can be called the sensation of verticality. Horizontal lateral, horizontal sagittal, and all other directions are groups of sensations reached by series of progressive changes from the verti-

cality-group. The errors of verticality made in passing from the vertical group to other groups are errors in estimating the amount of these sensations.

"Vertical" means the presence of a certain group of sensations. We are turned heels over head once every twenty-four hours as the earth revolves, and yet "vertical" does not change. We fly around a railroad curve with outer rail inclined. If the speed is just suited to the radius of curvature, our sensations remain the same, and the car appears to be vertical although we tip with the car; if the speed is too great, we apparently tip outward and if too small, inward.

In this discussion of bodily space we have rigorously excluded our experiences by sight. Visual space and its relation to bodily space will be considered later; here the reader is to suppose that his eyes are closed or that he is in darkness.

Having the sensation of verticality established, we can readily specify up, down, front, back, left, and right, by referring various other groups of sensations to it. If we start from the sensation of standing, "up" and "down" mean along a line through head and feet or any line parallel to it; "front" and "back" mean position in or near a vertical plane passing through the forehead and the back; "left" and "right" mean in or near a vertical plane passing through the shoulders.

If we maintain the body without bending, as on the tilting-board, the sensations through the body undergo definite changes from verticality to any other position. A line drawn from feet to head would be the most general method of indicating the particular position. As, however, the body changes in form by bending, it is necessary to specify the axis of verticality more definitely. In general we use the axis of the trunk for this purpose. The body is said to be vertical, horizontal,

or inclined according as the trunk is vertical or horizontal, and the directions of the arms, legs, and head are referred to this direction. The knowledge—without sight—of the particular position is learned by the sensations of resistance, &c.

This would be all we could know of bodily space if we were immovably fixed in a chair like some of the heroes of the Arabian Nights whose lower limbs were

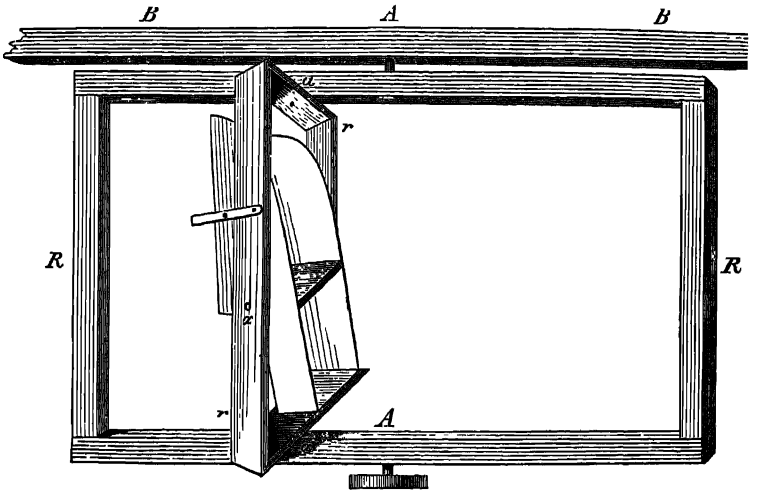


Fig. 92. THE ROTATION FRAME.

turned to marble. Our whole body, however, with its system of orientation can be rotated about any imaginable axis and can be transported in any direction and at any speed. Our notion of bodily space is thus enlarged by the idea of moving our system of orientation. Our notion of the movement we have undergone in a particular case may or may not agree with our notions in other cases. By secondary means we can eliminate disagreements and establish a standard space for move-

ment; then each particular result can be compared with the standard.

Let us investigate our notions of movement.

A large frame *R R* is hung on a vertical axis *A* (Fig. 92). Within this is a smaller frame *r r* on a vertical axis *a*, so arranged that the distance between *A* and *a* is adjustable.[1]

The person to be experimented upon sits in a chair resting on a horizontal axis *a*; the back of the chair can be placed at any angle to this axis. Thus the person can be tipped at any desired angle in any direction.

This horizontal axis is supported in the frame which swings around the vertical axis *a*. Thus the person in any position can be rotated around a vertical axis passing through the centre of the chair.

Since the frame *r r* is carried by the frame *R R* which rotates around the vertical axis *A*, and since the two axes are at adjustable distances, the person can be rotated around a vertical axis at any distance from the chair with or without rotation around *a* also.

Let us suppose a line drawn from the top of the head down through the body. Then with such an apparatus the person experimented upon is somewhat in the position of a planet whose axis may be inclined at any angle and in any direction to the ecliptic, which can rotate in either direction in the plane of the ecliptic (and therefore at any angle to its axis), and which can revolve in a circle of variable diameter around a centre. To avoid all judgment and dizziness from sight, a large pasteboard box is placed over the whole person.

The human planet in his box has his axis placed

[1] Mach, *Physikalische Versuche über den Gleichgewichtssinn des Menschen*, "Sitzber. d. Kais. Akad. d. Wiss., math.-naturw. Cl.," iii. Abth., lxviii. 122, Wien, 1874. Ibid., "Grundlinien der Lehre von den Bewegungsempfindungen," Leipzig, 1875.

vertically, and is set in rotation around it at a constant speed. The following facts are noticed :—At the very start he feels the rotation and knows its direction. In a very short time he can tell its speed fairly well. After a few seconds the feeling of rotation gradually disappears, and the person believes himself completely at rest. By slowly accelerating the speed the box can be rotated as rapidly as a top, while the person in it fully believes himself in perfect rest. If the speed is decreased, he believes himself to be rotated in the opposite direction. If the apparatus is suddenly stopped, he feels rotated in the opposite direction with a speed equal to the speed at which he was going. This apparent rotation gradually decreases. As the person cannot see what is being done, all these feelings of rotation and rest are supposed to represent the reality.

The rate at which a body moves around its axis is known as its angular velocity, *i.e.*, a perpendicular to the axis passes through a given angle in a given time. Any change in this velocity is known as an angular acceleration.

The laws governing our sensations of rotation are thus as follows :—A constant velocity in standard space appears as rest in bodily space ; a constant acceleration in standard space appears as a movement of constant velocity in bodily space.

With a constant velocity "here" appears to be a fixed place, regardless of the rapidity with which our system of orientation may be revolving.

In further experiments with Mach's apparatus the box is rotated steadily and strongly till no movement is felt, and is then suddenly stopped. The person feels a strong rotation in the opposite direction around the vertical axis. He now bends his head forward ; the axis

of rotation appears to bend forward also. When the head is at right angles to its former position, the person believes himself to be rotating around a horizontal axis.

If the person in the box holds his head at right angles during the original rotation and raises it when the rotation is stopped, he feels himself to be rotating sidewise and upward.

These and similar experiments show that angular accelerations in our rate of rotation depend upon the position of the head. The fundamental line of verticality is established by sensations from the trunk and limbs; the axis of acceleration is dependent upon the head (anatomically upon the semicircular canals); the relation between the two is determined (when the eyes are closed) by those tactual and muscular sensations by which we know the position of the head.

The human planet is now to be revolved around its solar centre. He is seated upright at one metre from the axis *A*, with his face toward that axis. The closed box is set in revolution about *A*. The movement is perceived as long as it is an accelerated one; as soon as it becomes constant, it is lost, and the person supposes himself to be at rest. The centrifugal movement, however, changes somewhat the bodily sensations that give us verticality, with the result that the body appears to be inclined slightly backward. As soon as the movement is stopped, the person feels himself return to the vertical. If the person is placed with his face at right angles during the experiment, he feels himself tipped outward. These and similar experiments show that a change produced in the bodily sensations by movement causes the sensation of verticality to change even though the axis of the body remains actually in the same direction.

In the previous experiment the person had, like the

moon, a fixed position in regard to the centre of revolution. By loosening a screw, the inner framework can be loosened, so that he can both rotate and revolve. If the box is now set in motion by a sudden impulse, the person revolves around *A*, but on account of inertia he remains facing in the same direction. After the first moments he does not feel the general movement around *A*, but only the progressive changes in his body sensations due to centrifugal force. This force apparently revolves around him, and he believes himself to be in the same place but to be revolving around some point in the body as if the body were a line whose movements produced the surface of a cone. In this experiment also the movement with constant velocity appears as rest, while the change in our bodily sensations appears as deviation from the vertical.

These phenomena are presented on a large scale by the movements of the earth. The rotation of the earth, its revolution around the sun, and the movement of the solar system, cause us to move in a most complicated path with terrific velocity; yet we are apparently perfectly at rest. The centrifugal force due to the rotation of the earth is opposed by gravity; our sensations of weight are due to the preponderance of the latter. On an earth of less density or with a swifter rotation our sensations would not be so strong, and likewise the reverse.

Since movements in straight lines can be considered as movements around a centre infinitely distant, it is to be expected that here, also, physical velocity appears as rest and physical acceleration as bodily velocity. The actual experimental proof was produced by Mach.

An inclined wooden track 22 metres long was built, with a fall of 2 metres, or 5° 12′. A car for the observer was connected by a rope over a pulley to a car for

weights; when one went up the other went down. The observer was enclosed in a box. As the observer was sent up or down, he perceived any acceleration or retardation as movement at a constant velocity; but a constant velocity was felt as rest. The phenomenon can be experienced on a rapid lift. The start and the stop (acceleration and retardation) are vividly felt, whereas in the middle of the journey the lift is apparently still.

Returning to the question with which we started: where are we? we must answer that our bodily space when referred to absolute space may be anywhere, and that up and down may be any direction; but that, as we know nothing directly of such absolute space, our actual space starts from ourselves. At any given moment we have sensations of motion and sensations of verticality. The former range from zero, or rest, onward; the latter indicate the angle at which the body is inclined. When we feel at rest, all moving objects in the universe perform their motions in respect to us. The earth at our feet is absolutely still, and sun and stars over us revolve. We are evidently "here" in every sense of the word, and other things are elsewhere. When, however, we feel ourselves in motion—no matter whether there is any motion relative to physical space or not—"here-ness" is attributed less to us and more to objects, the amount depending on our apparent rate of movement.

CHAPTER XXVII.

TACTUAL SPACE.

Our tactual experiences differ in intensity and in position. The property of intensity has been discussed as pressure (Chap. XX.); the property of position is the subject for present consideration.

When two slightly rounded points of hard rubber or ivory are simultaneously applied to the skin, they may appear as one point or as two points. The distance is found by which the two points must be separated in order to be felt as two. This is regarded as the threshold of tactual distance marked off by simultaneous points. It varies, according to the original experiments of Weber, from 1 mm. for the tongue to 68 mm. for the thigh.

The tactual sensations may be successive. Experiments on the threshold for distance with successively applied points have been made by Judd.[1]

A blunt bone needle weighted to 27 g. was allowed to rest for an instant on a spot (the middle of the volar side of the forearm), and then again on the same spot or on another spot above, below, or to the side of this one; the subject, not seeing the experiment, was to

[1] Judd, *Ueber Raumwahrnehmungen im Gebiete des Tastsinnes*, "Philos. Stud.," 1896, xii. 409. Full references to earlier experiments on the tactual threshold can be found here.

judge whether the same or a different spot was touched, and in the latter case to tell the direction of the change.

The procedure was that of successive steps, the fact of a change being known but its direction unknown.

The first result to be noted is that a certain average difference in the position of the spots touched is sufficient to make the sensations appear different in position, leaving the direction of the difference undetermined. With a still greater average difference the direction of the different positions becomes known.

This fact illustrates a general law governing our knowledge of sensations, which I may state as follows :—The degree of definiteness with which a sensation is recognised depends on the intensity of the sensation. This law I had occasion to notice five years ago in experiments on smell with an olfactometer, in which the intensity of the odour depends on the distance to which a small tube is drawn out of a larger one. I have described the experiment as follows :—

"In the whole range of psychology there is nowhere to be found a more striking method of illustrating the difference between the different thresholds of knowledge. As the smelling-tube is pulled backward the observer at first notices no odour ; the odour is said to be below the threshold. After a while he says, 'I smell something, but I can't tell what it is' ; a sensation is there, it is known as an odour ; it has passed the threshold of sensation but has not reached the threshold of recognition (if I may use such an expression). The odour becomes stronger and stronger ; finally the observer exclaims, 'Now I know the odour ; let me think a moment and I will tell you the name.' Very frequently he recognises the odour without being able to recollect the name. The difference between the threshold of sensation and the threshold of recognition is often considerable. If the odour is still further increased, the name, for usual substances, is readily recollected." [1]

[1] Scripture, "Thinking, Feeling, Doing," 125, Meadville, 1895.

Similar observations in various departments of mental life lead me to believe that the law is a universal one, and that it is the explanation of the fact noted by Judd in regard to tactual space.

Judd also made experiments in comparing the threshold of tactual distance marked off by simultaneous points (Weber's method) with the threshold marked off by successive points. The average for his five subjects was 24 mm. for the simultaneous points, and 7 mm. for the successive ones. This fact is an illustration of the general principle noticed by Weber that successive sensations of touch are more accurately judged than simultaneous ones.

In these experiments the points are separated by an empty interval. When the distance between two points is continuously filled, it becomes a line. The smallest distinguishable line-distance can be determined by applying the thin edges of cards. In Judd's experiments the cards were applied in four directions, lengthwise of the arm, crosswise, and slantwise 45° from right to left, or slantwise 45° from left to right. The subject had to tell when he felt the card as a line and in what direction it was placed.

The difference between the thresholds appeared here also, as is very evident in the details of the records. Edges from 2 mm. upward by steps of 2 mm. were applied in succession until the subject had judged correctly several times. A characteristic record is the following:

Line applied slantwise left to right (l.–r.).

Distance	2	to	6	felt as point.
„	8	„	10	„ blunt contact.
„	12			„ line crosswise.
„	14			„ line crosswise or slant r.–l.
„	16			„ line lengthwise or r.–l.

Distance 18		felt as line, direction unrecognisable.
„ 20		„ line, same as before.
„ 22		„ line, lengthwise or l.–r.
„ 24	to 30	„ line, lengthwise, at time with small variations to l.–r.
„ 32	„ 38	„ line l.–r. plainly.

Threshold for line distance 12 mm.; threshold for recognition of direction 32 mm.

The average threshold for line distance for the five subjects was 9 mm. The average threshold for recognition of direction varied from 12 mm. to over 50 mm.

In a similar manner the threshold of tactual space can be found for circles and other forms. Eisner, for example, found that circles of 2 mm. and 6 mm. could be distinguished on the back of the hand, and those of 2 mm. and 25 mm. on the back.[1]

The threshold of distance as marked by simultaneous impressions (Weber) depends on various conditions. Practice, hyperemia, sharpness of the points, rotation of one point, coffein, immersion in oil or water at skin temperature, &c., lower the threshold; fatigue, anemia, narcotics, pressure, distraction, and age raise it.[2]

The points of the line-distance may be successive and not simultaneous; this condition is produced by a point drawn over the skin.

For this purpose Hall and Donaldson[3] devised the kinesimeter, by which a point could be made to travel evenly over the skin at different rates.[4]

[1] Eisner, *Beurtheilung der Grösse und der Gestalt von Flächen*, Diss., Erlangen, 1888.

[2] References are to be found in Judd, as before, p. 453.

[3] Hall and Donaldson, *Motor Sensations of the Skin*, "Mind," 1885, x. 557.

[4] A greatly improved apparatus has been constructed in the workshop of the Yale Psychological Laboratory; Scripture and Titchener, "Am. Jour. Psych.," 1894, vi. 424; 1896, vii. 150, 151.

The moving point was applied to the skin, and the subject was required to state when he felt the motion, as up (toward the head) or as down. The time, *i.e.*, the length of the line drawn, and the decision were recorded.

The results, with rates of motion from 1.2 mm. to 150 mm. per second, showed that motion is recognised at an average distance of between 6 and 7 mm., regardless of the rate at which the line is drawn. It was also noticed in experiments on several persons that erroneous judgments in regard to direction were frequently made, the average being 14 %. The mistakes most frequently made were in judging the motion to be up instead of down.

"We are more likely when in doubt to judge motion on the surface of the limbs to be up rather than down their axis. On the breast, shoulder-blades, and back between them, the tendency was to judge movement to be towards the head, although these parts were less fully tested. Man's experiences with sweat and rain, especially without or before clothing, must have made him more familiar with downward than with upward movement on the surface of his body, and the latter, as being more apt to be caused by living things—insects, parasites, &c.— or by aggressive outward movements with the limbs, would be more likely to attract his attention. Movement also against the direction of the hairs, 'which strokes the wrong way,' would for anatomical reasons seem at first view to be a stronger stimulus than motion coinciding with their direction. Mainly, for this reason probably, *minus* or 'from' movement often failed to be felt with the lighter weights which in the opposite direction caused a distinct sensation. Whether the general law above stated holds for all parts of the surface of the limbs cannot be inferred on the basis of our observations, which were made mainly on the upper and inner forearm and on the middle of the upper thigh, but it seems not unlikely that it may for most of it." [1]

Up to this point we have spoken of experiments in which a certain difference in position is increased until

[1] Hall and Donaldson, as before.

it becomes noticeable; we have now to consider the curious case of a difference being noticed when it does not exist.

In experiments on the threshold of distance, according to Weber's method, it is customary to occasionally touch only one point of the compasses to the skin in order to get an unprejudiced judgment from the subject. It is soon noticed that frequently the subject states that

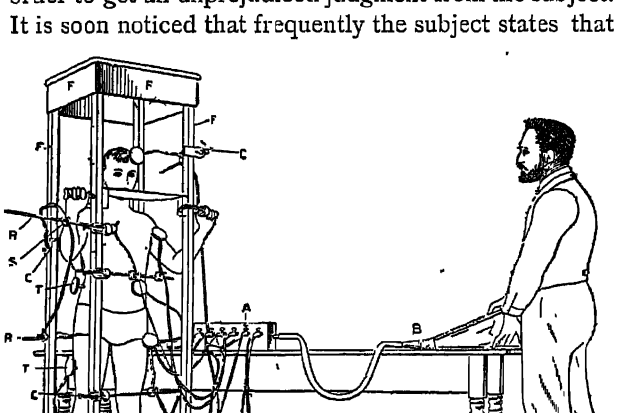


Fig. 93. APPARATUS FOR SIMULTANEOUS TOUCHES.

he feels two points when really only one has been applied.

In some experiments by Henri and Tawney[1] the subject was asked to estimate the distance between the two points felt by him. The sensations actually felt by the subjects when touched by the single point were: one point, two similar separate points, two

[1] Henri and Tawney, *Ueber die Tragwahrnehmung zweier Punkte, u.s.w.*, "Philos. Studien," 1895, xi. 394.

similar points connected by a line, two different separate points, two different points connected by a line ; or one distinct point with a light additional contact. The distance between the points has been estimated to be as great as 25 and 30 mm. in some cases.

The fusion of several touches into one and the diffusion of one touch into several are very prominent in some experiments by Krohn.[1] The subject stood within an upright frame (Fig. 93), in a steady position. Various rubber-covered boxes were so arranged that corks fastened to the rubber covers would touch the skin when air was blown into them. These boxes were connected by tubes to an air box with stopcocks ; the puff of air was furnished by a bellows. Any one or more of the boxes could be operated, and consequently any desired combination of touches could be made, *e.g.*, a group like the following : outside left ankle, outside right ankle, right side, left side, tip left shoulder-blade, tip right shoulder-blade, middle of forehead.

Characteristic fusion of sensations occurred as follows :

"There are two points stimulated on the front of the abdomen—one four inches to the right, and the other in the same plane four inches to the left of the navel ; the touches therefore being eight inches apart. Yet in three instances one of our best reactors, E. C. S., indicated but one touch on the abdomen, and that directly on the navel, midway between the two points actually touched. With J. A. B. there was also the same case of fusion. E. C. S. fused two touches, one at a point directly under the armpit on the right side, and the other at the tip of the right shoulder-blade—into one sensation, locating it at a point midway between them but lower down on the trunk. L. R. fused the two stimulations—at the top of the right shoulder and the tip of the right shoulder-blade, respectively—and localised the sensation as coming from a point midway between

[1] Krohn, *A Study of the Sense of Touch*, "Jour. of Nervous and Mental Disease," 1893, xviii. 169. The figure is reproduced, by permission, from the original monograph.

the two. In six instances the same subject fused into one sensation the two touches, at the back of the right calf behind and the outside of the right calf, in the same horizontal plane. Nearly every one of the ten subjects would, at the same time, in his individual series, fuse these two into one. It was also very common to fuse the two touches at the outside and front of the calf into one sensation, locating it as coming from a point a little lower but on a line between the two actual stimulations. One reactor felt as one and located at the knee (outside) the two touches upon the front of the right thigh and outside of the right calf respectively.

"There were also results diametrically opposed to the cases of fusion just cited. These cases of diffusion were, however, less frequent in occurrence and less striking in character. Thus, one subject being touched by a single cork, at a point immediately under the armpit . . . designated two localities about four inches apart. This case will seem the more remarkable when it is stated that in this same identical experiment ten subjects would, at the same time, [each] in his individual series, fuse these two into one. It was also very common to fuse the two touches at the outside and front of the calf into one sensation, locating it as coming from a point a little lower but on a line between the two actual stimulations. One reactor felt as one and located at the knee (outside) the two touches upon the front of the right thigh and outside of the right calf respectively. And still another subject, where there are two touches on the skin over each hip joint and two touches three inches on each side of the spinal column, three inches from the small of the back, did not feel these four stimulations properly, but experienced only two sensations and localised them as bilateral and on a point three inches directly back of the hip joint on either side.

"Mention must also be made of the cases of partial fusion in which two stimulations were felt as two, but at points much nearer together than the points actually touched. Thus, one subject localised the two touches which occurred at the tip of each shoulder blade as bilateral, but each was two inches nearer the spinal column than the actual points stimulated." [1]

The variations in intensity and space of the tactual sensations is one of the fundamental methods of gaining knowledge among the blind. Fingers, lips, and tongue are used to feel the objects presented to them.

[1] Krohn, as before.

The amount of the knowledge of form which a blind person can gain in this manner, can be summed up in the following way.[1] He is able to tell whether he has a round or an angular object in his hands, owing to the same or different intensities of pressure at different places. Again, he is able to state whether the object is regularly or irregularly shaped. To a certain degree he is able to tell the number of corners.

The way in which tactual space is developed among the blind is seen in cases of blind-born persons entering the asylum in an undeveloped state. At the beginning of their instruction in touching, says Scherer,[2] they are at a perfect loss what it all means.

The first activity which shows itself with these individuals consists in grasping the objects with the hand. The relations of form seem at first to be matters of indifference; the blind are much more interested in the various characters of the surfaces, *e.g.*, temperature, roughness, or smoothness. By a choice of appropriate objects made of the same material, it becomes easy to direct their attention spontaneously to the relations of form. The first distinction noticed is that between round and cornered; within these classes it is at first scarce possible for the blind to make any distinction. As the touch becomes more developed a change in the method of touching is noticed, whereby the knowledge from tactual space is supplemented by knowledge gained through sensations of resistance, heaviness, &c. The particular degree to which the tactual space and its accessories is developed depends upon the instruction and interest of the person. The blind handworker is always capable of perceiving space relations. On the

[1] Heller, *Studien zur Blindenpsychologie*, "Philos. Studien," 1895, xi. 252.

[2] Reported in Heller, as before, 439.

contrary, a blind musician, examined by Heller, showed an astonishing awkwardness in touching the simplest objects, and confessed that he did not have the slightest interest in the spatial relations of his surroundings. Another very intelligent blind person who had acquired



Fig. 94. LINE ALPHABET FOR TOUCH.

extensive linguistic knowledge had likewise not advanced beyond the lowest step of touch-development.

Tactual space is made use of for the purposes of communication. Various systems of letters for the sense of touch have been proposed. They fall into two classes,

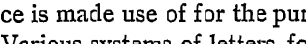


Fig. 95. BRAILLE ALPHABET.

namely, those with lines and those with points. In the one class the letters consist of lines produced in relief on the paper (Fig. 94). In the other they consist of points in different arrangements of space. The two most usual forms are shown in Figs. 95 and 96.

From the experiments on tactual space described in

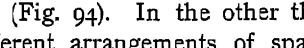


Fig. 96. NEW YORK ALPHABET (WAIT).

the previous pages, it is possible to decide which of the two kinds of alphabets would be the easier. The threshold of space for recognition of line-direction lay between 12 mm. and 50 mm. for the arm (p. 374), whereas for simultaneous points it ranged from 20 mm.

to 35 mm. (average 24 mm.), and for successive points 5 mm. to 9 mm. (average 7 mm., p. 373). It would thus be much easier to read a point alphabet than a line alphabet, provided the points were felt in succession. The actual experience in institutions for the blind has brought about a similar decision. The point alphabets consist of developments of a fundamental form. In the Braille system the fundamental form is ⁚, whereas it is ⁝ in the New York system. The forms for the first five letters are given in Figs. 95 and 96. Aside from various practical differences, such as the selection of the letter forms (*e.g.*, in the New York alphabet the most frequent letter, e, has the simplest form, and so on), it is to be noted that the Braille system throws more work on simultaneous distinctions, whereas the New York relies more on successive distinctions. We might suppose the latter to have the advantage in this particular; it would be rash, however, to express a definite opinion until extensive experiments have been made on the subject.

In reading the signs, the end of the index finger is passed over them in succession. The other fingers act merely as supports and are not taught to read. In advanced cases the finger of the left hand also assists whereby the right hand gives an outline sketch of the contents of the line, and the left hand fills up the details,[1] in much the same way as the side parts of the eye make the first sketch before the actual reading by the central portion.

Those who read most rapidly pass both fingers quietly over the lines, and the two hands differ only in the speed with which they do this. With some persons the hands separate in the middle of the line, the left

[1] Heller, as before, 458.

hand going to the beginning of the next line, while the right hand finishes. In most of the blind both fingers are equally educated; this is necessarily the case in reading music whereby the hands are alternately applied to the notes.[1]

[1] Heller, as before.

CHAPTER XXVIII.

MONOCULAR SPACE.

In considering monocular vision we should suppose one eye to be closed or covered, in order to be rid of the influence of binocular combination of the two monocular fields. We are also to distinguish as clearly as possible what we actually see from our knowledge, gained in various ways, concerning what is seen.

Looking at the world with one eye, we see the top of the ink bottle as an ellipse whose size and shading can be varied greatly by placing our hand into the field of vision and moving the bottle. In one position this ellipse becomes a circle; and in any position the ellipse can be made to increase or decrease in size by altering the muscular adjustments of the arm. For monocular vision, however, all conclusions regarding a constant shape, size, &c., are matters of inference, from relations to other senses. We can get some idea of what we actually see monocularly by supposing ourselves to be immovably fixed, and to have no other sensations than those of one eye, *i.e.*, no touch, no heaviness or resistance, &c. The whole world would then appear to us like the picture on the ground glass of a camera. Objects of various forms and colours would pass across the field; other objects would increase and decrease in size; still others would undergo changes of form and shading. We

might even by long experience and by the development of "monocular science" come to quite a "knowledge," connecting these phenomena together. We would then refer to the world, thus constructed, as the "real" world, and would consider our particular monocular world as merely our way of seeing the reality. In any case we must distinguish between the actual monocular world as we see it and the "real" world as constructed by inferences.

The monocular world is a purely mental affair. By dissections, experiments, &c., we are led to conclude that the monocular world corresponds to processes in the retina, in the optic nerve and in the brain; the term "retinal field" is sometimes employed to mean monocular world, but not quite justifiably. With the physiology of the retina, the psychologist has nothing whatever to do. As far as we are concerned, we may have no retina and no eye; we experience a complex of phenomena which we call our visual field. At the outset we may suppose the whole affair to be a pure hallucination. Indeed, a purely monocular person, such as the one we pictured above, would distinguish between "appearance" and "reality," whereas a distinction between "hallucination" and "appearance" would be something inconceivable. Possessing other senses and other means of inference, we mark off, as hallucinations of vision, those experiences that are not confirmed; but this distinction has nothing to do with the distinction between retinal and cerebral processes except as a deduction and not as a direct psychological fact.

In spite of the careful explanations of scientific investigators, this error of treating our monocular field as a retinal field occurs persistently. The general mind has become saturated with the error, and even among scientific men the problem still reappears: why do

we see things upright whereas they are bottom up on the retina? The confused condition of thought can be best cleared up by noting a few observations by Helmholtz.

"In the following exposition I prefer to substitute the two surfaces lying outside of our eyes in place of the retina and the retinal picture, because they are correcter expressions of our actual consciousness, and also because by directly placing all points in the two spherical fields we avoid the ambiguity of expression which has so often led to the error, that we know anything of our retina, of its size and extent, when we say that we judge the position of objects according to the point of the retina involved. It is moreover for all constructions on spherical surfaces completely indifferent, how large we make the radius. . . . We can also take the radius as negative, *i.e.*, we can place the spherical surfaces . . . where the retina and retinal pictures are. We can call such a spherical surface lying in the region of the real retina an ideal retina on which an ideal retinal picture lies. We must not suppose, however, that such a schematic retina corresponds to the real one in its dimensions, otherwise than as a very rough approximation. The real retina has an ellipsoidal form, and the retinal picture on it is in any case much distorted by assymmetries of the refracting apparatus. . . . In the normal consciousness of the seeing person the retina does not exist at all.

"When two bright points are present in the field of vision for the eye in a fixed position, two different optical fibres are stimulated by their light, and two sensations arise which must differ from each other by peculiar local signs because we are able to distinguish them in sensation. To which point of the retina these local signs belong, we know at the start just as little as where the optical fibres lie that carry them, and to what parts of the brain the stimulation is conducted. We can obtain information on the point only by scientific investigations; in regard to the part concerning the optical nerve and the brain, we have not got beyond the first introductory steps. Nevertheless, we know through daily experience how we must extend the arm in order to touch the one or the other bright object, or to conceal it from our eye. We can thus learn the direction of an object in the field of vision directly by such movements, and we learn directly to connect special local signs of sensation with the point in the field of vision where the object belongs. This is also the reason why we see objects upright in spite of their reversed retinal pictures. In fact, in the localisation of objects,

the retinal pictures do not enter into the consideration; they are only the means of concentrating the light-rays of each point of the field of vision in a nerve fibre. We have just as much right to wonder why the letters of a printed book are not reversed from right to left, because the metal letters from which it is printed are reversed." [1]

In fact, by the "retinal field" most writers really mean the "visual field" (which is not reversed, but upright, p. 417).

The fields of vision, monocular and binocular, are indeed purely mental affairs. They may be represented as spherical surfaces in relation to a solid, real world, as with Helmholtz and others, or they may be treated as groups of sensations having dimensions of different definiteness, as will be done in the following chapters.

There are two important landmarks in monocular space, the point of sharpest vision and the point of regard.

The *point of sharpest vision* is that place in the monocular field at which we can distinguish forms most accurately; it generally goes under the name of "point of direct vision." It is found by examining the visual field in regard to its distinctness, or sharpness, of vision.

The sharpness of vision can be tested by attempting to read type of various sizes. When the visual field is kept at rest, only a few words of the type in this page can be read, the rest is blurred. With very small type, or by moving the page away from the eye (whereby the test is made finer), this region of clear vision becomes reduced to a very small area, practically a point. This is the point of sharpest vision, or the point of direct vision.

Observations with stars, dots, and lines lying close

[1] Helmholtz, "Physiol. Optik," 2 Aufl., 680.

together, show that they must be separated by a distance of 60″ to 90″ in order to be distinguished at this point.

The *point of regard* is the point at which we are looking. If at the present moment I am looking at this dot (•), that is my point of regard. The two fundamental points in the field of vision are generally, but not necessarily, the same. Without changing the position of the point of sharpest vision, we can look at, or pay attention to, various other parts of the monocular field; the point of sharpest vision remains stationary, while the point of regard moves. Thus, while looking steadily at the dot, I can attend to the word above it, below it, &c.

It is this ability to separate the point of regard from the point of sharpest vision that renders possible the investigation of the whole extent of the monocular field.

When objects are moved outward from the point of sharpest vision, but are followed by the point of regard, they finally disappear; if moved inward from a place where they are invisible, they suddenly become visible. The "field of vision," therefore, does not extend indefinitely in all directions from the point of sharpest vision, but is a definitely bounded region. The point of sharpest vision is considered the "centre" of this region.

The boundary of the field of vision is generally determined by means of a perimeter. A white surface of a definite area, and a constant illumination, is moved out of and into the field of vision, along various radii from the centre, the distance of its point of appearance or disappearance being registered.

In doing this we have used the point of regard separately from the point of sharpest vision; we were

looking at, or paying attention to, something to one side of the centre.

Perimetry is thus a psychological affair; it consists in drawing the boundary lines of our field of vision. Let us proceed to make a map of this field.

In the first place, we put a dot on a piece of paper to represent the point of sharpest vision; then we draw

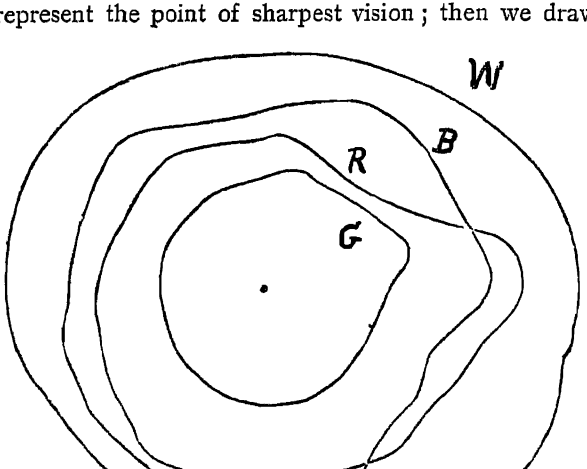


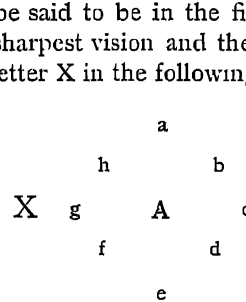
Fig. 97. FIELD OF VISION.

radii toward the edge on which to record our perimetry measurements.

Having the central point and the radii, let us examine the field carefully by moving an object in various directions. Beyond certain limits objects lose or change their colours; beyond further limits they are not seen at all. A characteristic map is shown in Fig. 97; green objects lose their colour beyond G, red

beyond R, and blue beyond B, while all objects disappear beyond W.

Hitherto we have spoken of a point of sharpest vision, a field of vision, and a point of regard. I wish now to call attention to the fact that there is also a "field of regard." When I fix my regard on an object, whether at the point of distinct vision or not, I am dimly conscious of the objects around it; these objects may be said to be in the field of regard. Let the point of sharpest vision and the point of regard be fixed on the letter X in the following diagram :—



All the letters of the A group are indistinctly seen and are dimly present in consciousness. Now, with the point of sharpest vision still at X, fix the point of *regard* on A; the letters of the A group are still just as indistinctly seen, but A is the most prominent character in consciousness, the other letters are less prominent, and X is the least prominent. All the letters can be said to be in the field of regard around the letter A.

I might have said "focus of attention" instead of point of regard, and "field of attention" instead of field of regard; moreover, instead of speaking of the movement of the point of regard, I might have used the expression "fixing the attention." As expressions of ordinary language these would have done very well, but as scientific terms they are hardly definite enough. The facts are as stated. There is a field of variously

coloured and variously formed objects, with one point at which they are most distinct, and with another point at which they are most prominent. These two points generally coincide, but often do not.[1] The measure of the distinctness at any spot is the just noticeable difference in space when that spot is made the most prominent. The measure of prominence or degree of regard might be found in the change in this just noticeable difference due to more or less prominence.

In neurasthenic, hysterical, and other persons the perimeter often gives results that show a strongly contracted field of more or lest permanence of form. Measurements on children also show a field somewhat contracted.[2] This has generally been stated as a contraction of the field of vision; the eye is supposed to be blind outside the contracted field. I venture the suggestion that such a person is not blind to objects outside the field, but sees them in the same way as we do those parts of the visual field to which we do not attend. In other words, the perimeter measurements give the limits of the field of regard and not of the field of vision. These persons cannot direct their attention so far to one side. Perimeter measurements demand a separation of the point of regard from the point of distinct vision; this requires an effort which in children would be not quite so successful, and in hysterical persons would be far less successful. When an object suddenly enters the field of vision, it arouses

[1] In young children the connection between the two points has to be established by trial and practice. The attempt at connection can be seen when a bright object is shown to one side of the child's point of sharpest vision. He will try to move his eye so that this point is brought to cover what has attracted his "attention," or rather his point of regard.

[2] Luckey, *Comparative Observations on the Indirect Colour Range of Children, &c.*, "Am. Jour. Psych.," 1895, vi. 489.

regard, and by the fact of association of the two points the eye tends to turn toward it. With a limited field of regard, or field of attention, objects within the field of vision, but far from the centre, would not attract regard and would apparently produce no effect.

Some experiments of mine seem to support this view.[1] Cards, each containing a picture in the middle and a letter in the corner, were shown to the observer for such brief intervals that only the picture could be seen. After a set of four or five such cards the letters alone were shown.[2] The person was to tell which of the pictures first came into his mind on seeing a letter. One of the series used was this :—1. peacock (F) ; 2. shield (A) ; 3. cat (I) ; 4. flag (: :) ; 5. negro (C). A specimen result would be :—1. I, cat ; 2. : :, flag ; 3. A, shield ; 4. C, negro ; 5. F, ——. Although the letters had not been seen before, yet in four cases out of five when they were seen they suggested the appropriate picture. As proof of the fact of such unconscious seeing and associating of objects, I give the results of my experiments in the following table.

Five persons were experimented upon. The first column gives the person ; the second, the number of cards shown ; the third, the percentage of correct associations made ; the fourth, the percentage to be expected from chance ; and the fifth, the fraction showing the relation of fact to chance.

TABLE.

I.	15	0.27	0.20	$\frac{27}{20}$
II.	48	0.39	0.21	$\frac{13}{7}$
III.	45	0.20	0.20	$\frac{1}{1}$
IV.	25	0.36	0.20	$\frac{1}{5}$
V.	39	0.50	0.20	$\frac{5}{2}$

[1] Scripture, *Ueber den associativen Verlauf der Vorstellungen*, "Philos. Studien," 1891, vii. 136.

[2] The method of experimenting was described on p. 205.

The number of experiments is not large, but it is sufficient to establish the fact.

My explanation would be that, although the letters were not "seen" in the sense of "noticed," yet they were seen in the sense of being present in the field of *vision;* the field of *regard* was not large enough to include them, and there was no time for moving it.

There is one portion of the field of vision in which we see nothing; this is the "blind spot." Holding this book about seven inches from the eye, close the left eye and look fixedly at the cross.

The circle will disappear, although A and B are still seen.

The shape of this blind surface may be readily determined. With the eye looking fixedly at a point on white paper placed in a definite position, the inked point of a bright pen is brought to a position where it disappears in the blind spot; it is then carefully moved outward, and its position when first seen is readily marked. This is done on all sides until the marks are sufficiently close for the outline to be drawn.

Although we have proven ourselves to be blind at this spot, nevertheless it does not appear as a black hole in everything we look at.

How is this hole filled? What do we see with the blind spot?

When the circle is made to disappear, the space between A and B remains entirely white. No matter what may be the colour of the circle or of the field in which it is placed, whenever the circle is made to disappear, the colour of the field is spread over the place occupied by the blind spot.

When the field does not consist of a single colour

but of two or three colours the blind spot is filled by the two colours with the dividing lines running directly between them. If a more complicated figure is used there is confusion and perplexity; the spot is not filled out with anything definite, but is apparently occupied by an indefinite something of no particular form or colour. It is not like the space behind our backs, where we see nothing, for even in the most puzzling cases, the blind spot simulates any strong change in general illumination or colour of the surrounding field. Even on a printed page the blind spot simulates the dotted appearance that would be produced by letters.[1]

We might now proceed to investigate, at each point in the field as compared with the centre, the sharpness of vision, or the sensitiveness to weak lights, or the changes in colours, &c. Let us, however, take up the following problem: how does our monocular space compare with standard space in regard to distances?

In regard to the physical world we are accustomed to believe that a yard is always a yard. In the psychological world this is not true; a horizontal yard is quite different from a vertical or any other yard. Let us put the question in a scientific form: a given distance laid off to one side of the point of sharpest vision is assumed as a measure, how does this distance change for other parts of the visual field?

Place a point on a sheet of paper held squarely in front of the eye; measure off a distance of one inch outward—*i.e.*, towards the temporal side. Now, keeping the point of sharpest vision steadily at the original point, mark off equal distances above, below, and inward. These are equal distances in the monocular field. Applying the standard measure to these distances, we

[1] A collection of blind spot cards is to be found in Bradley "Pseudoptics," Springfield, 1894.

find that they do not measure exactly an inch. Since we see the same measure moved from one to the other, and since our knowledge of it as obtained in various ways is sufficient proof that it remains a constant quantity, we are forced to believe that equal spaces in our monocular field are not equal spaces in our standard space.

But why not say that equal spaces in the visual field are not physically equal? This is just the point to be avoided. I *see* certain distances in my visual space to be equal; experience by vision, by touch, &c., and inferences drawn from it, lead me to *assume* the particular inch measure as an unvarying standard. It might change its length as I turned it; in fact, one could be very readily constructed to do so. If every material and every process used for such measures did change in this way, I should not know the difference and should still assume the measure as the standard in spite of its change. If this change happened to agree with that in my visual field, the latter would readily be assumed as a standard space.

Suppose that we proceed to lay out, at various points, distances equal to the starting distance; in this way we could construct a map of the monocular field referred to itself as a standard. The result would differ from the general standard. Vertical distances would be smaller in the monocular field than in the standard; vertical distances above the centre would be shorter than those below; horizontal distances would be pretty nearly equal; finally, vertical and horizontal directions would not coincide exactly with the standard vertical and horizontal.

A familiar example of the fact that upward distances are shorter than downward ones is found in the letter S and the figure 8, in which the upper half is apparently

equal to the lower, until they are turned bottom up, S and 8.

But something still more curious is to come. The distances in the monocular field depend upon the way they are filled. If instead of distances marked by points we draw lines, the difference between horizontal and vertical becomes less and our map is changed.

Not only this ; the apparent equality depends on how adjacent parts of the field are filled. If the two distances are used as lines for the sides of a square, the difference is still less ; if they are used as points in the circumference of a circle, the difference disappears entirely. Again, the filling of the space, the directions of adjacent lines, &c., influence the results. Thus the map changes continually.

We have just been speaking of comparing distances. In order to illustrate the methods employed in accurate psychological comparisons of distance I will describe an investigation by Merkel[1] into what is called "measurement by the eye." The investigation takes one of the particular cases I have mentioned ; it is, however, not confined to the determination of the lengths compared, but inquires into the psychological factors coming into play.

The person experimented upon was seated looking down on a black horizontal line drawn on white cardboard (Fig. 98). The line was cut in the middle by a fine piece of steel ; two other pieces of steel cut off portions on each side. The observer thus saw an indefinitely long line with three fine cross lines, of which the middle one was at the point of sharpest

[1] Merkel, *Die Methode der mittleren Fehler, experimentell begründet durch Versuche aus dem Gebiete des Raummasses*, III., "Philos. Studien," 1893, ix. 400.

vision. Without any movement of the eye he was to compare the two pieces of line cut off by the cross pieces. The outer pieces were movable by means of micrometer screws turned by the wheels at the ends of the apparatus. These wheels were so arranged that the lengths of the lines were indicated in thousandths of a millimetre.

By this means a line of a definite length is presented at one side of the middle and a line equally or

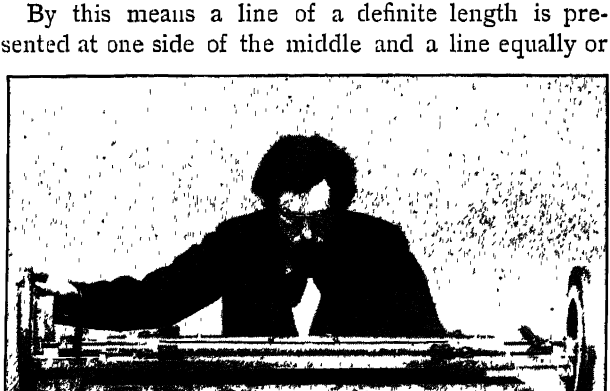


Fig. 98. COMPARING DISTANCES BY THE EYE.

unequally long was to be marked off. Several problems now arise.

The first problem is to find the just imperceptible difference between the standard line N and the varied line. Starting from a line B which is visibly too short to be equal to the standard, we gradually increase it till at M'_u it appears equal to N. The amount by which it falls short of real equality, $d'_u = N - M'_u$, is the just imperceptible difference towards smallness, or, let me call

it, the lower just imperceptible difference. (In Fig. 99 the relation is represented on an exaggerated scale.) Starting with a line C evidently too long, we diminish it till at M_o' it appears equal to the standard N. This gives $d_o' = M_o' - N$ as the upper just imperceptible difference.

The next problem is to find the just perceptibly different line. Starting with the two lines just alike, we decrease the variable line till it appears just

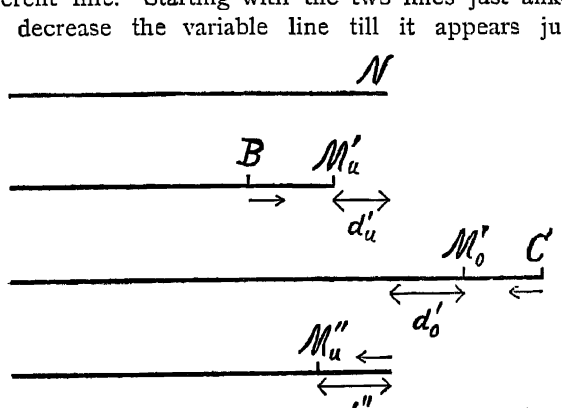


Fig. 99 DIAGRAM TO ILLUSTRATE THE JUST IMPERCEPTIBLE AND THE JUST PERCEPTIBLE DIFFERENCES.

smaller than the standard. We have M_u'' as the just perceptibly smaller line, and $d_u'' = N - M_u''$ as the lower just perceptible difference. In a similar manner we obtain M_o'' as the just perceptibly longer line, and $d_o'' = M_o'' - N$ as the upper just perceptible difference. It must be clearly understood that Fig. 99 expresses the judgment of the lines from a psychological point of view. The actual measurements may give entirely different results—even negative quantities.

Psychological interest does not end with the just perceptible and imperceptible differences; it desires a calculation of the mean variation of the results obtained for each of these. The value given for d'_u, for example, will be the average of all the records for d'_u in the whole series of experiments; the average fluctuation of the single values around this average will be obtained as described on page 47. By making the apparatus sufficiently accurate these mean variations become purely psychological quantities indicating the insecurity of judgment. We thus have the four quantities F'_u, F'_o, F''_u, F''_o representing the fluctuations in the values of d'_u, d'_o, d''_u, d''_o. With these values we can determine several things. Let us take the results for a single standard distance, say, 1 mm., for which Merkel obtains—

$d'_u = -0.002$	$d'_o = -0.028$	$d''_u = 0.057$	$d''_o = 0.026$
$F'_u = 0.007$	$F'_o = 0.007$	$F''_u = 0.009$	$F''_o = 0.006$

The lower "least" perceptible difference is the average of d'_u and d''_u, or $d_u = 0.028$; the upper is similarly $d_o = -0.001$. The minus sign of d'_o indicates that the just imperceptibly larger line was really smaller than the standard, owing to the direction of the change from C downwards (Fig. 99); this gives a small minus difference in the result.

Let us ask what was the average least perceptible difference from the standard; it was the average of all four, or $d = 0.013$. Let us ask what was the most accurate method for comparing lines. We see that the smallest error was with d'_u; therefore the most accurate way was to begin with a line B (Fig. 99) and increase it till it appeared equal.

The regularity of judgment was about the same in all four methods of comparison, the values of F being practically the same.

Suppose now the experiments to extend over the distances from 1 mm. to 50 mm. We can then trace the influence of the length of the line on the just perceptible and imperceptible differences and on the certainty of judgment. In Merkel's results the two differences generally increase with the length of the line; in one form of judging, namely, M_o', there is a steady increase as a negative quantity. The uncertainty of judgment steadily increases as a very constant fraction of N. Curiously enough, there is no simple proportionality for the values of d while there is a very close one for F.

In conclusion, it is perhaps necessary to call attention to the fact that the just imperceptible difference, described on p. 397, is quite a different mental quantity from the just perceptible difference going toward the standard, described on p. 291; the former involves a positive judgment of equality between two things whereas the latter records simply the failure to detect a difference.

Optical illusions of the various kinds have, as matters for curiosity, long been subjects for amusement and speculation. Lately they have attracted the ardour of the investigator. Here, as elsewhere, science was the resultant of curiosity mixed with patience.

Suppose we investigate the effect of marking off the ends of a line by cross lines at different angles. The fact that such cross lines affect the apparent length of the main line has long been known, but a satisfactory explanation was first rendered possible by a systematic investigation.[1]

[1] Heymans, *Quantitative Untersuchungen über das "optische Paradoxon,"* "Zt. f. Psychol. u. Physiol. d. Sinn.," 1895, ix. 221. I give the account somewhat in detail as a good example of a systematic search for facts and explanations.

The apparatus is made of cardboard—a rectangular piece, *a b c d* (Fig. 100), measuring 25 × 15 cm. ; there is placed a band *a' b' c' d'*, and on this two pieces *a'' e'' f'' d''* and *g'' b'' c'' h''* ; they are pasted together in such a way that similar letters are over one another. When the piece *i k l m* is pushed under the top pieces,

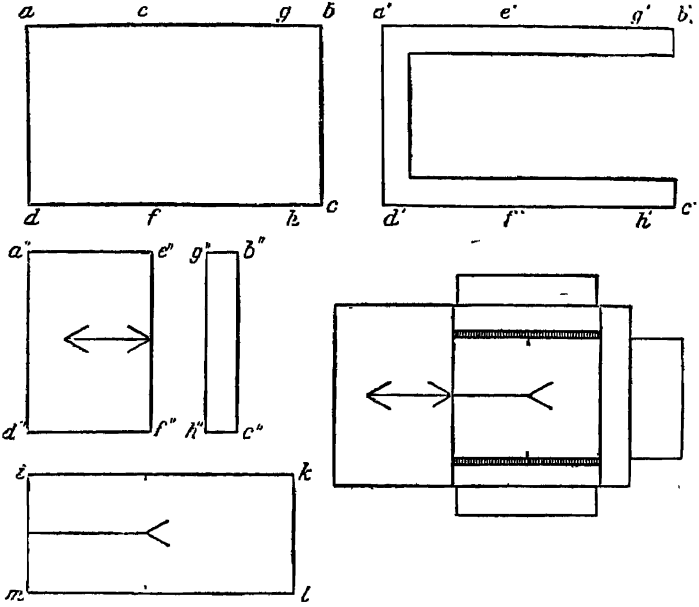


FIG. 100. PARTS OF THE LINE ILLUSION BOARD.

the result looks like the complete diagram shown also in the figure. To enable measurements two millimetre scales are placed on the edges ; they are covered by paper flaps when an experiment is being made, and are uncovered, as shown in the figure, when the result is to be read.

The apparatus shows a horizontal line divided into

two parts by side lines meeting at an angle. The two halves are made apparently equal by pushing the cardboard *i k l m* with the variable line under the piece to the left with the constant line. The problem to be investigated is the effect of the angle lines with various lengths and inclinations on the distances between them. For each length of angle line and each size of angle a new pair of drawings has to be made.

Let the length of the angle line be made 20 mm. and the length of the constant line 75 mm., while experiments are made with various angles. The results of Heymans's experiments are shown in the table, which gives the angle made by the angle line with the horizontal and the average number of millimetres by which the variable line was made shorter than the other—

Angle	10°	20°	30°	40°	50°	60°	70°	80°	90°
Average illusion ...	18	17	17	15	14	11	8	3	0

In this particular case there is a curious constant relation between the amount of the illusion and the cosine of the angle.

The length of the angle lines also has an influence on the amount of the illusion. Let us take 75 mm. as the length of the constant horizontal line, and 30° as the constant angle, and repeat the measurements with different lengths of angle lines. Heymans's results were as follows :—

Length of angle line	2.5	5	7.5	10	12.5	15	17.5	20	30	40	43 3	50	60	0
Average illusion	3	6	10	12	13	14	16	16	17	15	13	12	11	10

With increasing length of angle line the illusion increases to a maximum and then decreases. This holds true for all angles, but the maximum changes its place

with a change in the angle. The results of experiments similar to those just stated, but with various angles, show that the maximum effect for an angle of 10° is obtained with 27.5 mm. for the angle line, for 30° with 30 mm., for 50° with about 40 mm., and for 70° with from 50 to 80 mm.

What influence has the size upon the illusion? Up to this point the constant horizontal has been made 75 mm. long. Now let us take different lengths for this line. By proportionately increasing the angle lines

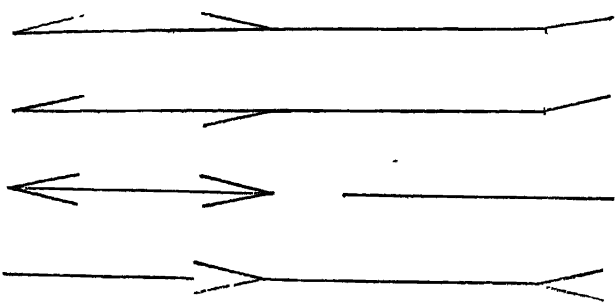


Fig. 101. ILLUSION FIGURES WITH ANGLE LINES LACKING.

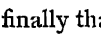
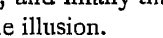
also the whole figure is simply enlarged or diminished, the angle being kept constant. The results obtained by Heymans are as follows:—

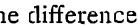
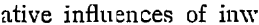
Length of constant line...	25	50	75	100	150
Average illusion	6	12	18	22	31

The increase in illusion is almost proportional to the size of the figure, but it drops off slightly for large figures.

But the figure has three pairs of angle lines; what would happen if some of them are omitted? The following constants were assumed: horizontal line

75 mm., angle line 20 mm., angle 10°; and four figures were used (Fig. 101). The results gave 12 mm., 12 mm., 5 mm., and 9 mm. as the average amounts of illusion for the four figures respectively beginning with the upper one. It thus appears that it is quite indifferent whether the three angle lines are all on one side or the middle one on the opposite side to the end ones. Also that the angle lines that are turned outward are more influential than those turned inward; and finally that the omission of angle lines decreases the illusion.

The difference between the results for the two lower figures in Fig. 101 suggests a further investigation into the relative influences of inward to outward angles in determining the maximum point of illusion. Two sets of experiments were made; in the first, the constant line had inward angles  and the variable line had none; in the second, the constant line had none and the variable had outward ones . The constant line was 75 mm., the angle 30°, and the angle lines 15, 30, 45, or 60 mm. The results were as follows:—

Constant line	Variable line.	Angle lines.	Average illusion.
	———	15 mm.	5 mm.
,,	,,	30	6
,,	,,	45	7
,,	,,	60	7
———		15	8
,,	,,	30	11
,,	,,	45	9
,,	,,	60	7

It is seen that when a line without angles is compared with one with inward angles , the illusion shows no tendency to decrease even with an angle line

of 60 mm., whereas when a line with outward angles >——< is compared with a line without angles a tendency to decrease is noticed even with angle lines of 45 mm. We may therefore conclude that in a complete figure the appearance of a maximum point of illusion, with the following decrease, is to all effects exclusively produced by the outward angles > <.

Suppose we inquire into the grounds for the illusion. It has been stated that the reason why we over-estimate or under-estimate the lengths of the lines is that we pay attention to the areas included between the angle lines. Thus the total area included in an inward figure

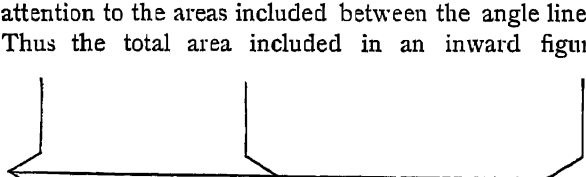


Fig. 102. ILLUSION FIGURE WITH INCREASED AREAS.

<——> is less than that in an outward figure >——<, and we really judge the whole area rather than the line in the middle. There are various deductions to be made from such a hypothesis which disagree with the experiments reported. But the matter can be made the subject of direct experiment. If we pay attention to the areas, the addition of lines as in Fig. 102 ought to increase the illusion as the included area is greater. Using as constants, standard line, 75 mm., angle line 10 mm., angle 30°, the results give as the average illusion: regular figures, 12 mm., Fig. 102, 10 mm. That is, the result is just the opposite from what would be expected if the hypothesis were true.

Let us try again. If the area or additional imaginary lines are influential in the illusion, the result would be greater if the area were made more prominent by actual lines. Three figures (Fig. 103) were used, with

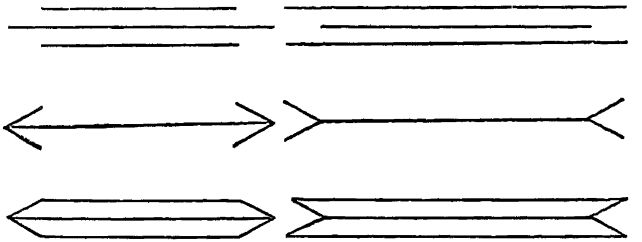


Fig. 103. ILLUSION FIGURES WITH AREA LINES

constants as before; the results were 4 mm., 11 mm., and 12 mm. respectively as the amount of illusion. Thus the additional lines were of very small influence and the essential part is evidently not in the area, but in the angle lines.

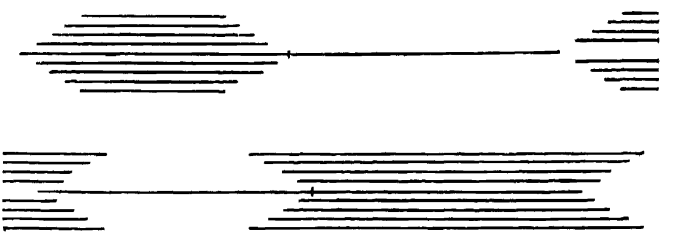


Fig. 104. ILLUSION FIGURES WITH FILLED AREAS.

Let us fill the entire area with lines as in Fig. 104. We get the results (constants as before except angle line 20 mm.): 6 mm. and 8 mm. for the illusion, while the original figure gave 14 mm.

Again, let the area be filled entirely with black or with white whereby the hypothesis would require a like result for both figures. The result gave (constants as before except angle line 10 mm.), 9 mm. and 13 mm. respectively.

The hypothesis evidently falls completely.

Another hypothesis asserts that the illusion is due to the over-estimation of acute angles. According to such a hypothesis the illusion would reach a maximum at 30°, and would increase steadily with the length of the angle line; this is refuted by the experiments. Moreover, a direct experimental refutation can be made. Take the two Figs. 105 where there are no acute angles at all. The results gave 8 mm. and 10 mm.

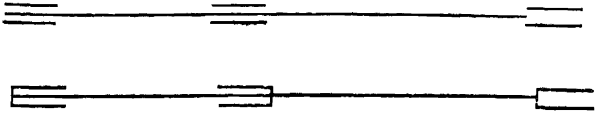


Fig. 105. ILLUSION FIGURES WITH NO ACUTE ANGLES.

Still another hypothesis explains the illusion by involuntary, forced movements of the point of regard. In his experiments Heymans was accustomed to place the constant line to the left of the figure. Happening to turn the figure around, he noticed that the illusion which had previously decreased owing to practice, reappeared with fuller force than ever. He also noticed a surprisingly irresistible impulse to follow the angle lines, especially the middle ones. Such impulses are undoubtedly present; how could they bring about the illusion?

The fact of a most favourable size for the angle and the angle lines seems to indicate that two forces are at work, one to assist the illusion, the other to suppress it.

These we can find in a contrast between movements. As the eye starts to follow the horizontal line, it gets an impulse to movement at the angle also. If the angle opens in the direction of the eye-movement the horizontal component of this side-movement will agree with the intended movement; if the angle opens in the opposite direction, the horizontal component will be opposed to it. The actual horizontal movement afterwards executed will appear in the former case smaller and in the latter larger by contrast. This effect would be a factor favouring the illusion and depending on the angle. But this applies only to the angle lines at the beginning; the other pair of angle lines gradually comes into vision as the eye proceeds. The effect of the end lines is opposed to that of the starting lines. Since the angles are equal and opposed, we should expect no illusion.

It is readily noticed, however, that the first angle has its full effect, while the second begins its work only at the end; the first angle consequently determines the presence of an illusion. Now, as the angle lines increase in length, the illusion, as due to the first angle, should increase. It does, until the lines reach a certain length, after which the amount of illusion diminishes. This point is explained by the fact that with short angle lines the first angle can be seen more nearly alone, whereas with longer lines the second angle pushes itself into notice and diminishes the illusion. There must consequently be a maximum point where the influence of the first angle, as increasing with length, begins to increase less rapidly than that of the second angle. The matter depends on the distinctness with which the two are noticed. With inward angles both are from the beginning about as distinct as they ever will be, and the gain of the first over the second will remain about constant.

In conclusion, a test for this theory is planned. By lengthening the horizontal line, the second angle is moved further away from the first; consequently the length of the angle line must increase much more before the second angle gains over the first, and the maximum appears later. Experiments were made with horizontals of 50 mm., 75 mm., and 100 mm., and an angle of 30°. The results gave a maximum for the 50 mm. line at 20 mm. of angle line, for the 75 mm. line at 40 mm. of angle line and for 100 mm. at 60 mm. of angle line. The results are such as demanded by the theory.

The whole field of the "optical illusions" opens up before us. A general view of the subject can be found in Helmholtz's "Physiologische Optik." Unfortunately, very few researches have been made[1]; our knowledge still remains largely general, and experimental psychology has little to contribute.

Up to this point we have treated our monocular field as a flat space; is it anything more?

When objects are moving around in our visual field, we notice that one frequently hides another. For example, some one passes a red sheet of paper sidewise toward a blue one; if you know nothing concerning the relative sizes and can keep out deductions from other objects, *e.g.*, by looking through your hand bent like a tube, you cannot tell which will hide the other till it actually happens. We might say that one object pushes the other out of existence. So it does, as far as our

[1] Kundt, "Ann. der Phys. und Chem.," 1863, cxx. 118. Aubert, "Physiologie d. Netzhaut," 266, Breslau, 1865. Knox, *On the Quant. Determination of an Optical Illusion*, "Am. Jour. Psych.," 1894, vi. 413. Thierry, *Ueber geometrisch-optische Täuschungen*, "Phil. Stud.," 1895, xi. 307, 603, xii. 67. Burmester, *Beitrag zur experimentellen Bestimmung geometrisch-optischer Täuschungen*, "Zt. f. Psychol. u. Physiol. d. Sinn.," 1896, xii. 355 (full references).

monocular field is concerned; the hidden object is simply not there. By feeling around with our hands we can grasp the two objects at once; they both really exist, yet only one of them is present in our monocular field.

Any one who has read the romance of "Flatland" will remember the story of A Square who lived in the world of two dimensions where there was nothing but length and depth. A Sphere coming into Flatland out of the third dimension appeared as a circle of varying size. When the Sphere rose, as we would say, he simply disappeared out of existence.

Our monocular field is also a flatland. Things appear and disappear, increase and decrease in size. The end of a receding railway-car seems to shrink up, and, if we had no knowledge by inference, we could not say that it "receded," but merely that it grew smaller. If we put our hand before an object, it is simply gone; it has disappeared out of flatland.

Our flatland, however, differs from Square's Flatland by consisting of length and breadth, not length and depth. To Square his country appeared as a lineland, but was flatland because he could walk about in it and because he learned to make inferences from shading, perspective, &c. To us the monocular field appears primarily as a flatland laid out like a map before us.

This view of the world as a flatland is characteristic with persons who have lost one eye while retaining the other. On page 243, experiments were described that were made on a young man who had lost one eye. This subject reported that a few months after the removal of the eyeball, the wounds healed and his left eye became serviceable. At first he had great trouble in seeing objects near him. He could not fix his eye

[1] A Square, "Flatland," Boston, 1891.

for the near objects. He could not estimate the distance from one object to another behind it. In looking from the third story of a building in which he was working, the sidewalk seemed to be level with the street.

I have said that our flatland has no depth. This is not exactly true; from various experiences we actually manufacture the ghost of a third dimension. The subject just referred to relates that after a few months he learned to determine depths almost as accurately as before. What was it that gave him a third dimension? Let us illustrate by an experiment.

Into a rod of the form shown in Fig. 106, several pins are driven so that their heads are almost, but not

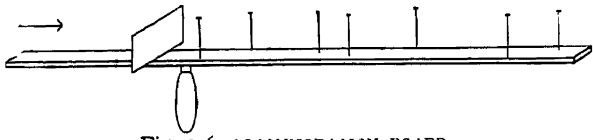


Fig. 106 ACCOMMODATION BOARD

quite, in a line. The small screen hides the bases of the pins and nothing but a group of heads is seen by the eye, looking in the direction of the arrow. One of these heads will appear sharply defined; the rest will appear misty, or blurred. At will we can make any other one of the heads appear sharp, whereby the previously sharp one becomes blurred. We cannot, however, tell how far off the pins are. If we attempt by sight to place the finger on one of them, it falls quite in the wrong place. Of course, as the finger does or does not cover the view of the pin, we know at once which is the nearer. We likewise know from the comparative sizes of the pins that some of them are further away than others, that none of them are 50 feet away,

&c. Finally, for near objects the sensations involved in accommodation of the eye give us some idea of their distance away. In this way we have some idea of depth, just as the one-eyed person has. This idea of depth is not a feeling of space like the length and breadth of monocular space or the depth of binocular space. With only one eye open we can distinctly feel the sensations involved in accommodating from a distant wall to a finger held at arm's length, yet, as far as any vision of depth goes, the finger appears to be on the wall. The statement that the monocular field lacks the third dimension does not mean that all things are localised in a definite plane; our flatland is a land of definite length and breadth, but of an utterly indefinite depth.[1]

[1] Hillebrand, *Das Verhältniss von Accommodation u. Konvergenz zur Tiefenlocalisation*, "Zt. Psych. Phys. Sinn.," 1894, vii. 97. Arrer, *Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung*, "Phil. Stud.," 1896, xiii. 116, 222 (full references to previous work, particularly to the opposed views of Hering and Wundt).

CHAPTER XXIX.

MONOCULAR SPACE AND BODILY SPACE.

In the monocular field we find two sets of lines constantly recurring, those that have the same direction as the horizon and those that have the same direction as the trees, the course of freely falling bodies, &c. We may call these directions horizontal and vertical; the terms refer to ocular space, and need not necessarily stand in any relation to the horizontal and vertical of bodily space (p. 363).

We have thus the foundation for a system of orientation in the ocular field, namely, the vertical and horizontal lines always present. The point of sharpest vision can be made to traverse these or other lines.

This movement of the point of sharpest vision lies in a space of practically two dimensions (the third dimension, representing the feeling of accommodation, being indefinite and negligible, p. 411). This form of the statement involves no hypothesis concerning the action of the eye muscles and does not go beyond the bare facts. Regarding the cardinal directions in the monocular field as the axes, we can express the movements of the point of sharpest vision in the usual way by use of co-ordinates.

Suppose the eye to be looking at a surface so large that no objective boundaries are present, the visible

surface being limited by the limits of monocular field. Let the surface be marked off with the axes X and Y. We will start with O at the point of sharpest vision (Fig. 107). Suppose this point to move through the distance O I. The monocular field changes to a new position with I as its focus. We

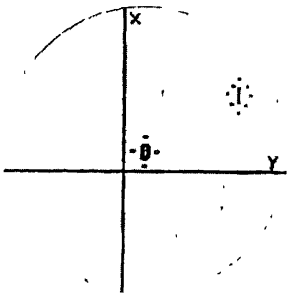


Fig. 107. MONOCULAR FIELD WITH POINT OF SHARPEST VISION AT O.

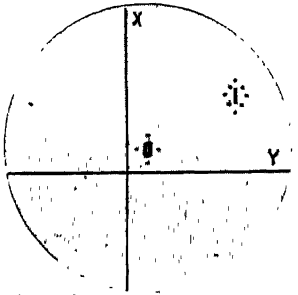


Fig. 108. RESULT OF CHANGING POINT OF SHARPEST VISION TO I BY EYE-MOVEMENT.

can distinguish two kinds of results. In one kind the limits of the field remain nearly as before for all moderate changes of the focus; the nose, eyebrows, &c., are seen as fixed in relation to X, Y. (Fig. 108). This we call a movement of the eye. In the other case the limits of the field move in regard to X, Y; there are at the same time sensations from the neck, and we say that the head is moved (Fig. 109). Of course, the two can be combined in any way.

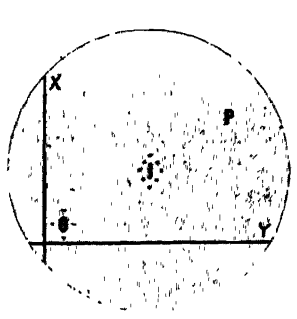


Fig. 109. RESULT OF CHANGING POINT OF SHARPEST VISION TO I BY HEAD-MOVEMENT.

Let us denote the eye movement of the monocular field by H, whereby we mean the sum of all our

experiences when the point of sharpest vision has been moved from O to I with the limits of the field practically unchanged. The quantity H is what we know as the voluntary movement of the eye. Physiologically this corresponds to a movement of the eye-ball by the eye-

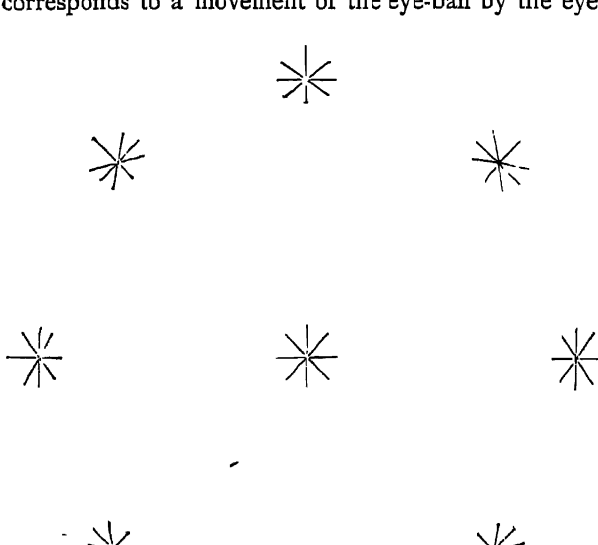


Fig. 110. APPARENT CHANGE IN THE SYSTEM OF ORIENTATION AS THE EYE IS MOVED.

muscles; mentally it is composed of sensations from the muscles, &c., and of the visible changes in the field. "Movement of the eye" is psychologically the factor H.

In geometry the system of co-ordinates is considered as fixed, regardless of the movements of a point within it. Is this the case in monocular vision?

By looking steadily at a coloured cross in the point of sharpest vision we can force its image, in its complementary colour, to move as the point moves. We can thus carry a portion of the monocular field from one place to another.

To perform the experiment a double cross of coloured paper is placed in the middle of a large board with cross lines at right angles. This board, suspended at its centre, is placed so that the X axis is horizontal. The point of sharpest vision is held on the cross for a few minutes and is then moved outward successively in various directions. The result is indicated in Fig. 110. The general law for the dependence of the system of orientation on the position of the point of fixation can be thus stated: if the original system be regarded as a fixed plane, the system after movement will appear on this plane as the projection of a system in a plane rotated with respect to the first around an axis perpendicular to the direction of the movement.[1]

It is noticed that a given movement of the point of fixation produces a constant change in the system of orientation. If we move from the point A to the point B, and back again, we find the system at A to be as we left it. Likewise, if we move from A to B and then from B to C, we find the system at C to be the same as we should have found it by moving from A to C. The relations of the systems of orientation are therefore constant—which, in the midst of a change of system for every movement, would seem to be a gain in simplicity.

In stating the law of dependence of orientation in movement I spoke of the original system "regarded as

[1] This law is roughly true; for rapid or extreme movements and under special conditions there are deviations from it; particulars are to be found in Helmholtz, "Physiologische Optik," § 27 and following.

a plane." In the illustrative experiments it was a plane, and the double cross actually appeared to be distorted. In our ordinary experience we do not see such distortions. We can move the cross around over the sky without any noticeable distortion. When we move the point of sharpest vision from one point to another over the scene before us, we do not see a continual expansion and contraction of the objects in view. Indeed, the view seems to remain practically constant in shape and arrangement, both with movement and without. The conclusion is evident: we do not see the monocular field as a plane.

To determine the actual form of the monocular surface we can ask: What form would give an unchanging system of orientation? The answer would be: The inner surface of a sphere. In fact, we do regard the upper half of the monocular field, when out of doors, as a spherical vault, namely, the sky. Likewise, to persons on a mountain or in a balloon, the earth appears as the inside of a bowl. The form of the sphere may not be perfect. Owing to various influences the sky is flattened till the edges are much further away than the centre. The third dimension still remains very indefinite under all circumstances, yet there is a hollowness about all our monocular views, indoors and out, although the form may not be spherical.

By moving an object around in our monocular globe we can find its centre with considerable definiteness. Not far from this centre we find a fixed object, the point of the nose. We also find a human body not far away. A few tests soon convince us that this visible human body is to some extent connected to the felt human body with its touch sensations, &c. We naturally suspect that there may be some connection between bodily space and monocular space.

There is an actual relation between the two spaces. We can tell—without sight—the position of the body, *i.e.*, of the trunk (p. 364), as vertical, horizontal, or inclined. We can also—without sight—move our hand in a vertical, horizontal, or inclined direction. If, now, we open the eye, we can see that the ocular vertical coincides with the bodily vertical, *e.g.*, a drop of water runs lengthwise down the trunk, or the hand moves in the path of a falling ball, and that the ocular horizontal coincides with the bodily horizontal, *e.g.*, we see the moving hand to coincide with the horizon. We thus have the beginnings of a system of orientation for the ocular-corporeal space.

Moreover, we know—bodily—the meanings of front, back, sidewise, &c. A tap on the chest is located—bodily—as "front." With the eye we can see the instrument strike the blow. We can thus see the directions known bodily as front, sidewise, and (partially) back. At the same time we see that the monocular field lies in front of and (referred to the trunk) somewhat upward from the body.

In the next place, we can move the point of sharpest vision with or without moving the centre of the monocular sphere. When we do move the centre, we experience certain sensations from sinews, joints, and skin, which—in bodily space—indicate to us movements of the head in relation to the trunk. Thus, although the monocular field is changed by every movement of the head, we always know its relation to the fundamental bodily space.

The fixity of the monocular field in relation to the body is not always maintained.

Let us suppose ourselves on a rotation board (Fig. 111).

Our monocular field is, say, a ceiling with gas-fixtures,

&c. The board is started in rotation. The ceiling apparently remains fixed in position, and we feel our body moving. The original system of orientation of bodily space remains constant, and our body appears by its changing sensations to change its position in regard to it. The monocular field actually rotates but this visible rotation is corrected by reference through the bodily sensations to the original position. Suppose, now, these bodily sensations to fail. The

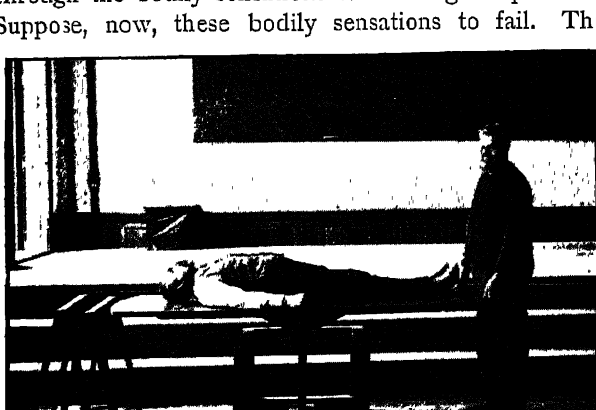


Fig. 111. ROTATION BOARD.

ceiling then appears to rotate. Such rotation (or other movement) of all or part of the monocular field in reference to bodily space, with bodily sensations of movement absent, is considered to be "objective" movement when confirmed by our other means of determining it and "illusion" of movement when not so confirmed.

The experiment has been varied by Warren.[1] The

[1] Warren, *Sensations of Rotation*, "Psych. Rev.," 1895, ii. 273.

room was darkened. The subject lay on the rotation board with head slightly raised and eyes screened to permit of only a small area being seen towards the feet. This area was covered by a screen at the feet ; a square hole was made through the screen. Two strips of white paper were hung on the opposite walls of the room.

When the strips were not visible the subject felt the usual sensations from being rotated, with the usual result (p. 367) that a steady rotation was finally felt as rest. The strips were now made visible in a faint light. The subject's internal sense of rotation prevailed and the strips seemed to flit by or move according to judgments from this sense. With a stronger light the visual sensations prevailed. The slightest movement of the body was detected. With the eyes closed there was a sensation of reversed movement when the rotation was stopped ; this was now checked by the sight of the strips. When a mirror was inserted in the screen so that the strip behind the head was seen and was therefore apparently moved in the opposite direction to the movement of the head, the subject thought himself to be moving sidewise (not rotating) in the direction in which the head was going.

CHAPTER XXX.

BINOCULAR SPACE.

Most of us possess two monocular fields, the left-hand one and the right-hand one. These two fields are alike in general outlines but differ in particulars. Looking through the window with the right eye closed, we see the window bars crossing the scene over the way; looking with the left eye closed we see the scene practically as before, but the bars cross it in different places. Closer examination shows us that the two fields, although in the main alike, differ everywhere as to particulars of size and arrangement. This becomes very apparent when the eyes are opened and closed in rapid succession.

What happens when both eyes are open? Our two flat monocular fields differ from each other; when both are combined into one binocular field the general outlines are as before, but the field receives an entirely new character, that of depth or relief. By combining two slightly different fields from flatland, the result becomes a model in spaceland. The laws that govern this combination form the problems of binocular vision.

We will suppose two monocular fields to be present with their points of sharpest vision. We will disregard the unusual cases where the scene for one eye is different from that for the other; and will suppose the two mon-

ocular fields around the points of sharpest vision to be alike, say, a door. Although we see a door with each single eye, we see only one door with both eyes. This is the fundamental fact of binocular vision; namely, the union of two monocular views into one binocular view. There is no question of *why* to be asked any more than there is *why* red and yellow make orange. It is a matter of fact in either case, and psychology is concerned with determining the laws that govern such combinations.

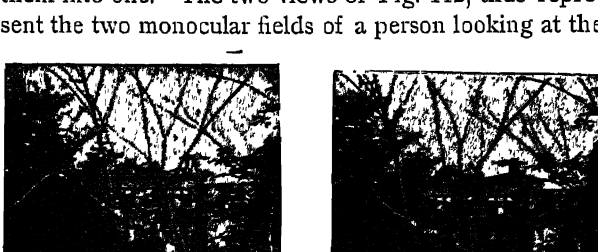
While we are looking at the door, the sunlight suddenly shines on the floor. When a new object appears, it generally becomes the point of regard, and the point of sharpest vision of each eye at once moves from its previous position to the new point of regard. Again, the table, the chairs, &c., successively attract the point of sharpest vision. The passage from one object to the other is accompanied by particular sensations of sight and muscular movement known as "movements of the eye." Such sensations from a single eye did not give us what we now notice: a distinct, definite, irresistible feeling of depth.

The varying movements of the two points thus build up the whole field of binocular depth with a system of distances from zero to infinity in all visible directions. We thus at any moment definitely locate any object we look at in binocular space. When the two points of sharpest vision are the same, the single point is called the "point of fixation."

What is the relation of the other points of the binocular field to the point of fixation?

Let us start with two monocular fields. Fig. 112 shows a pair of views taken by a stereoscopic camera. The stereoscopic camera contains two lenses a few inches apart on the front board of the camera box. A

partition in the box divides it into two distinct compartments. When we compare the two views on the ground glass of the stereoscopic camera, we find them to differ practically in the same way as the view differs for us when we look at it with one eye successively in the two places occupied by the lenses. It is this principle that enables us to photographically record the two monocular scenes just as they would appear to us if we did not combine them into one. The two views of Fig. 112, thus represent the two monocular fields of a person looking at the



L R.

Fig. 112. TWO MONOCULAR VIEWS FOR THE LEFT AND RIGHT EYES RESPECTIVELY.

scene presented to the camera. The point of sharpest vision is at the window of the house in the distance, as indicated by the black dot. It will be observed that in reference to this point the other parts of the picture are differently situated in the two views.

Disregarding, now, the manner in which these views were obtained, we will consider them as what we actually see with the left and right eyes singly. What happens when the two are combined?

The most effective method of producing monocular fields experimentally and then combining them binocu-

larly is that of stereoscopic projection. Of several forms of this method I have found the application of polarised light to be the best.[1]

Light is said to be polarised when its vibrations occur in only one plane. This condition is accomplished in several ways. One of them is to place a bundle of parallel glass plates at an angle of 55° in the path of the beam; part of the light is deflected sidewise, being polarised in one direction, and part passes onward through the plates, being polarised at right angles to the other. Let such a bundle of plates, called a polariser, be placed in front of the lens of a projection lantern. The light reflected sidewise is destroyed by the blackened surface of the casing, and the picture will be thrown forward on the screen in light polarised in one particular direction.

To the eye this picture does not differ from that in ordinary light, although the light is polarised in one plane, say vertically. The term "vertically polarised" indicates the readjustment of the light around a vertical axis, but does not imply that the vibrations occur vertically rather than horizontally, this fact remaining still undetermined.

Let us now place another polariser, turned at right angles to the first, in front of another lantern. The picture thrown by it on the screen will be in horizontally polarised light. To the eye, pictures in this light do not differ from those in vertically polarised or in unpolarised light.[2]

[1] Weinhold ("Physikalische Demonstrationen," 341, Leipzig, 1881) suggested the use of polarisers, analysers, and a bronzed screen for stereoscopic projection, but added that no one would attempt to carry out the idea in earnest. The method has been made successful by John Anderton, of Birmingham, England, whose apparatus I have described here.

[2] In order not to destroy the polarisation of the light a silvered screen is used.

We place the two polarisers in front of the lenses of a biunial lantern (Fig. 113), and throw two pictures at the same time. If the two pictures are alike, one single picture appears as usual. Now place the view Fig. 112 L

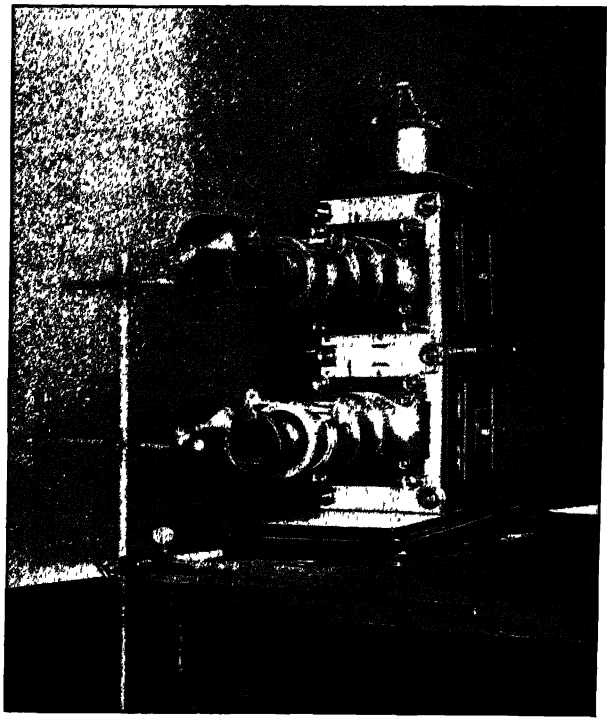


Fig. 113. THE LANTERN STEREOSCOPE.

in one lantern, and Fig. 112 R in the other; the result is a confused mixture of both views (Fig. 114). When the two points of sharpest vision, indicated by dots, are made to coincide, no other points will do so; the branches, for example, are quite some distance apart.

The two views thus physically mixed on the screen are now to be received into the eyes separately. For this purpose we use the pair of analysers (Fig. 115). They are composed of bundles of parallel glass plates, having the same effect as those of the polarisers. One of the analysers allows only the vertically polarised light, the other only the horizontally polarised light, to pass through it. We thus see with the left eye the picture of the left field only, and with the right eye the picture of the right field only. The result is remark-

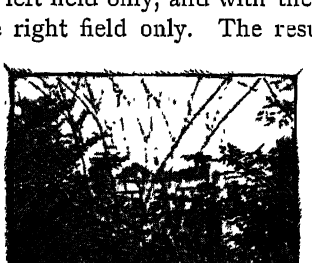


Fig. 114. MIXTURE OF THE TWO MONOCULAR VIEWS OF FIG. 112.

able. We do not see two different flat pictures as they really are. We also do not see a confused picture as it appears on the screen. What we do see is a single picture with all the points coinciding perfectly, but with an entirely new property to it which we experience in no other way. This property is "binocular relief." The picture is no longer on the flat screen, but is a picture in relief of exactly the same kind (excepting the colouring) as the objects we see in the world around us.[1]

[1] Another method of stereoscopic projection throws the two views in different colours and employs eyeglasses of corresponding colours. It can be conveniently used in place of the polariser

In illustrating the process of binocular vision I use this method of building up solid pictures by means of flat pictures on the screen, because it makes evident to the observer that binocular relief is not necessarily a property of objects themselves, but that it consists in a binocular combination of pictures that differ according to certain laws.

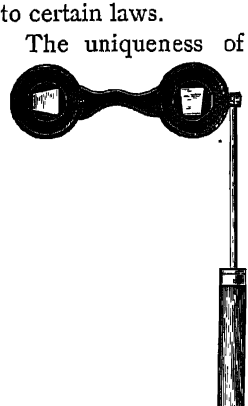


Fig. 115. EYEGLASS FOR THE LANTERN STEREOSCOPE.

The uniqueness of binocular relief can also be brought out by looking at the objects in front of us with one eye and with two eyes alternately. Although we may be perfectly acquainted by sight and touch with the chair and the table before us, there is always a marked and specific change in the character of the picture when we change from monocular to binocular vision. No amount of monocular and tactual education can produce binocular relief; the person with one eye must always remain as ignorant of this experience as the colour-blind person of full colour vision.

To illustrate the laws governing binocular relief, let us take a pair of views (Fig. 116). The points of sharpest vision F, F are made to coincide. The points A, B, C, D also coincide. The point G in the left field

method whenever good lanterns are at hand; it does not require a special screen, and the total cost is a trifle. I have described various technical points in regard to this method in the "Scientific American," 1895, lxxiii. 327. To the account there given I would add the recommendation of the use of sheet gelatine instead of glass when lime-light is employed.

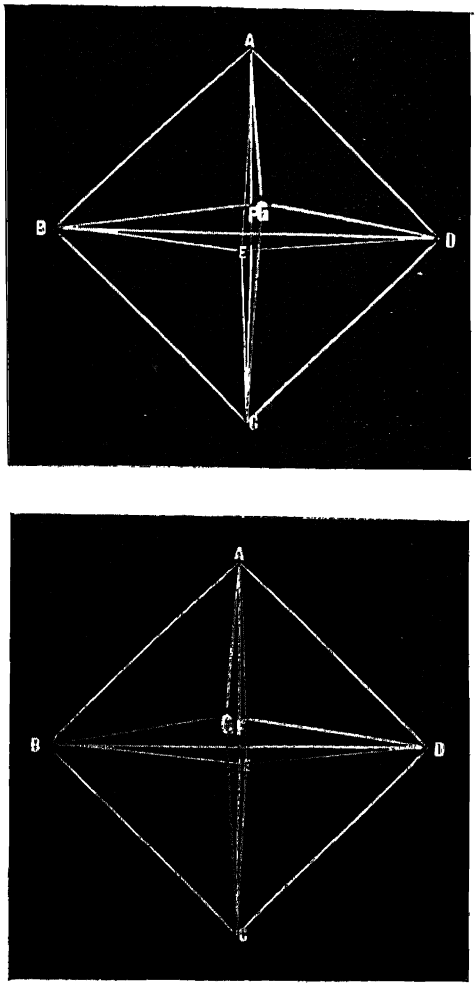


Fig. 116. BINOCULAR FIGURES TO ILLUSTRATE CROSSED AND UNCROSSED DISPARITY.

is to the right of F, while in the right field it is to the left. This condition is called "crossed disparity." With other points like E the point of the left field is to the left of that of the right field; this is the condition of "uncrossed disparity." When we look at this pair of views on the screen, the point G is seen to be further in front than F, and the point E to be further behind; in general we can say that crossed disparity places a point nearer, and uncrossed disparity places it further away.

In the foregoing experiments the point of fixation has remained fixed on the screen. The binocular depth was thus constant at the distance of the screen, and the binocular relief was a transformation of a flat surface into a solid figure at that depth. We thus have two qualities of binocular space in addition to those derived from monocular space; these are—(1) depth, which corresponds to differences in movement of the point of fixation; and (2) relief, which corresponds to the union of disparate points of the monocular fields.

The law governing the amount of binocular relief can be expressed in the following way. In order to avoid negative signs we will suppose ourselves to be looking at a distant point, *e.g.*, the stars or the horizon, whereby the point of fixation is at a practically infinite distance, and whereby also there is no disparity except crossed disparity. If a stereoscopic picture of such a scene be taken and the two views be laid together so that the distant points coincide, all objects in the left-hand picture will appear further to the right than in the right-hand picture. The amount of this disparity is called the stereoscopic parallax.

As the point of fixation is changed from infinity, the distant points become double in steadily increasing uncrossed disparity; while the previously double points

steadily diminish in crossed disparity. The stereoscopic parallax for the infinitely distant points, which was formerly zero, becomes a steadily growing negative quantity, while the positive parallax of the nearer points decreases.

What is the relation of stereoscopic parallax to binocular depth?

We will start with the point of fixation at infinity; the two monocular fields for this position we will name the fundamental monocular fields. We will suppose these fields to have systems of co-ordinates with the origins in the points of sharpest vision. For each point of one field there is (except where the field is lacking) a point of the other field with the same values for its co-ordinates; such pairs of points are "identical points." All objects infinitely distant will occupy identical points. Nearer objects will not occupy identical points. The amount by which the actual points differ from identical ones is the amount of disparity.

As the point of fixation is moved nearer, the identical positions in the two fields of vision will no longer be occupied by infinitely distant objects, but by nearer ones. For every change in the position of the point of fixation the identical positions will be occupied by different systems of objects. The sum of all the objective points occupying identical positions for a given position of the point of fixation is called the "horopter" for that position. The determination of the form of the horopter is a purely mathematical problem.[1] Some of the particular cases are as follows: With the point of fixation at infinity directly in front

[1] The equations for the form of the horopter were stated by Helmholtz in 1862; the first published statements concerning its form in the simpler cases were made by Hering in 1863.

the horopter is a plane perpendicular to the line from the point of fixation to the face. Mathematically this plane is at an infinite distance; practically, since the eye cannot distinguish minute differences, the horopter is a space covering all objects beyond a certain limit. With the point of fixation directly in front, but at a distance nearer than infinity, the horopter is a circle lying in a plane passing through the eye and the point of fixation and passing the point of fixation and the centres (for the lines of direction supposed to be drawn from objects to the eyes) of the two eyes, and also of a perpendicular line erected at the point of fixation. For all other positions of the point of fixation the horopter is the same as in this case, as long as the point is not raised or lowered from the horizontal plane extending outward from the eyes. When the point is on the floor directly in front the horopter line likewise lies on the floor. For other positions the horopter becomes more complicated.[1] The importance of the horopter lies in the fact that points lying in it are more readily united by the two eyes.

The "fixation surface" is a curved surface whose points occupy, in the two monocular fields, positions which are identical horizontally, regardless of possible vertical disparity. Careful experiments[2] have established the law: the distance from the fixation surface of any point seen singly in binocular space depends only upon the amount of disparity between the points of the two fields. That is, the position of any point in regard to it is determined by the points

[1] For the determinations see Hering, "Der Raumsinn und die Bewegungen des Auges," 377, in Hermann's "Handbuch der Physiologie," and Helmholtz, "Physiologische Optik," 2te Aufl., 860.

[2] Hillebrand, *Die Stabilität der Raumwerte auf der Netzhaut*, "Zt. f. Psychol. u. Physiol. d. Sinn.," 1893, v. 1.

it occupies in the fundamental monocular fields, and is utterly independent of the position and form of the fixation surface.

Up to this point we have attended only to the space elements in binocular vision ; it is interesting to inquire concerning the experimental study of points moving in three dimensions just as we studied their movement in two dimensions (Chap. VI.). Dvorák,[1] Sanford,[2] and Münsterberg[3] have developed the stereostroboscope for this purpose.

"The principle of the stereoscope is to have the two eyes converging to one point, and yet see two different pictures. If these two pictures represent a solid object as it would be presented to the right and left eyes respectively, they are perceived as one solid object. The instrument shown in Fig. 117 consists of two discs united by a steel rod, which may be rotated rapidly. The distance between the discs may be varied, to give any desired degree of convergence.

"The front disc is made of heavy black cardboard, having in it two concentric circles of radial slits. The inner and outer slits alternate at 30° from each other. The observer sits before this disc with the eyes on a level with the slits when they are horizontal, so that one circle of slits will pass before his right eye and the other circle before his left as the disc rotates.

[1] Dvorák, *Ueber Analoga der persönlichen Differenz zwischen beiden Augen und den Netzhautstellen desselben Auges*, "Sitzb. d. kgl. böhm. Ges. d. Wiss. in Prag," 1872, Jan.–Juni., 65.

[2] Sanford, *Notes on New Apparatus*, "Am. Jour. Psych.," 1894, vi. 576.

[3] Münsterberg, *A Stereoscope without Mirrors and without Prisms*, "Psych. Rev.," 1894, i. 56. In choosing the Münsterberg apparatus for description I was not guided by any questions of priority or completeness of invention, in regard to which I am not sufficiently informed to express an opinion. The following account was prepared for me by Mr. E. Lough, of Harvard. The block for Fig. 117 was obtained from the "Psychological Review."

"The back disc, which rotates with the front one, contains a series of twelve figures 30° apart, whose middle points are 16.5 cm. from the centre of the disc. These figures are stereoscopic pictures, so placed that the right eye looking through its slit sees only the right-hand view and the left eye only the other.

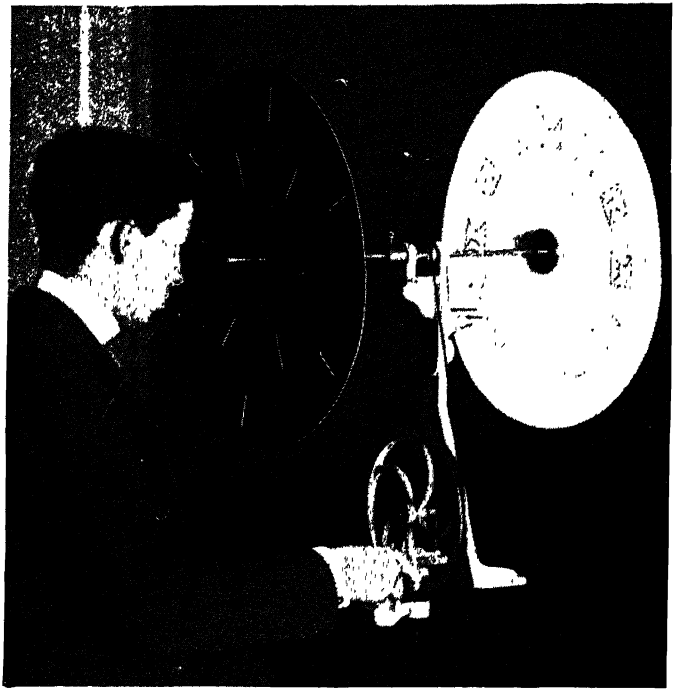


Fig. 117. THE STEREOSTROBOSCOPE.

"With each rotation of the disc six views are presented to each eye. When the rotation is so rapid that these images fuse into continuous impressions the condition of simultaneous stereoscopic vision is fully attained. If,

now, these figures vary slightly, after the manner of those used in the stroboscope (p. 111), the effect of a solid moving object results."

The apparatus thus applies, in the first place, Dvorák's principle[1] of binocular union, which is as follows: Two successive impressions reaching the eyes separately are perceived as simultaneous, provided the difference does not exceed a certain limit. When the two impressions are made to appear in the same place this limit is about $\frac{1}{12}$ sec. The second principle[2] is: When the two impressions are such as would be required for relief vision, the relief is actually seen. Thus, instead of having the two monocular views presented simultaneously, as in viewing real objects or the ordinary stereoscopic views, they may be presented successively. It is not difficult to see the great advantage of the method in some cases. In most persons the acts of accommodation and convergence are insolubly connected. With the points of sharpest vision of the two eyes at the horizon each eye is accommodated also for the horizon. As the points are brought to meet on a nearer object, the accommodation is likewise changed to suit. In the ordinary stereoscope, however, the two pictures are placed beside each other, and, as the points of sharpest vision must be placed on the corresponding points of the pictures, they are practically in the same relation as if placed at the horizon. Thus the two views of Fig. 112, when placed in a stereoscope, require the points of sharpest vision to be about 50 mm. apart at a short distance from the face, which is the distance required when the points unite for a very distant object. The accommodation is thus adjusted for a distant object, and the near pictures appear blurred. With Dvorák's method,

[1] Dvorák, as before, 70. [2] Ibid., 70.

as well as with the method of stereoscopic projection previously described, the pictures occupy the same place, and the convergence and accommodation are adjusted to the actual distance of the object. The difference of method is well brought out by the case of one man who, on account of excessive myopia of one eye, had never been able to successfully see stereoscopic figures in the usual stereoscopes, but who saw them perfectly when shown in Münsterberg's apparatus.

The third principle involved is that of kinetoscopic, or stroboscopic, vision, which has been described in Chap. VI. The result is a movement of objects, not only on a plane surface, but also in depth.

The successful development of the kinetoscopic method for showing moving pictures on the screen leads us to hope for its combination with some method of stereoscopic projection. With the stereoscopic method employing polarised or coloured light a double vitascope, accurately synchronised, would be required. Dvorák's method would require only one apparatus, with which the two views are presented alternately. This would involve the change of the polarisers or colours at each exposure, a problem which does not present insuperable difficulties. When this is accomplished, the entire equipment for the presentation of a play—as far as concerns the eye—will consist of a "kinetostereoscope," a flat screen and the eyeglasses for the audience. The actors will come, go, advance, and retreat on an apparently real stage having true depth. The colouring of the pictures is to-day accomplished without great difficulty. One thing alone is lacking: the words and music. This can be supplied by a phonograph synchronised with the pictures. This has actually been attempted for the kinetoscope, as

described on p. 116. The union of the three methods—kinetoscopic, stereoscopic, and phonographic—would not only make it possible to reproduce everywhere an opera or a play, but would also enable future generations to see and hear, in practical reality, the actors, singers, and musicians of former ages.

PART V.

PAST AND PRESENT.

CHAPTER XXXI.

AN INEVITABLE EVENT.

THE fundamental difference between the science of the Greeks and the science of to-day lies in the introduction of methods of careful observation, of statistical calculations, of measurement, of experiment, and of mathematical deduction, in the place of superficial observations as the basis of a fabric of speculation. For Aristotle a few observations of nature were sufficient for the most widely-reaching conclusions. For the other Greek philosophers even these few observations were not necessary; pure thought was amply sufficient for the attainment of all knowledge worth having. The revival of learning brought about an adoption of the Greek methods of thought which was applied to the science of the Middle Ages.

The blow delivered at this fabric by the heliocentric theory of Copernicus and the later astronomy was followed by the development of the method of careful observation by Galilei. The tremendous change thus brought about can be understood by comparing the methods of ascertaining astronomical knowledge preva-

lent in Galilei's time and those of to-day, as is illustrated by the following extract from Francesco Sizzi, a Florentine astronomer, who argues thus against Galilei's discovery of Jupiter's satellites :—

"There are seven windows in the head, two nostrils, two eyes, two ears, and a mouth ; so in the heavens there are two favourable stars, two unpropitious, two luminaries, and Mercury alone undecided and indifferent. From which and many other similar phenomena of nature, such as the seven metals, &c., which it were tedious to enumerate, we gather that the number of planets is necessarily seven.

"Moreover, the satellites are invisible to the naked eye, and therefore can have no influence on the earth, and therefore would be useless, and therefore do not exist. Besides, the Jews and other ancient nations, as well as modern Europeans, have adopted the division of the week into seven days, and have named them from the seven planets ; now if we increase the number of the planets this whole system falls to the ground." [1]

Of course I do not mean to imply that Galilei was the first to make careful observations ; he was, however, the most prominent and powerful figure in this new method of acquiring knowledge. From astronomy the method has gradually worked its way into various departments of thought.

The introduction of this method into the study of mind came through Hobbes, who was strongly influenced by the teachings of Copernicus, Kepler, Harvey, and Galilei. After Hobbes there arose a succession of brilliant English thinkers, such as Locke, Berkeley, Hume, Hamilton, and Mill, in whose hands the method received the furthest development to which it could possibly be brought as long as it was confined to the study of the person's own mind.

It is greatly to the credit of English psychology that the victory of observation over speculation was won

[1] Lodge, "Pioneers of Science," 106, London, 1893.

there long before elsewhere. It was not until the beginning of the present century that it occurred in Germany. Then it happened as a revulsion from the widely spread speculative philosophy. The attack on the speculative method was successfully made by Herbart. In place of speculation the material of psychology was to be found by "internal observation, association with persons of various degrees of culture, the observations of the educator and the statesman, the expositions of travellers, historians, and moralists: finally, experiences with the insane, the sick, and with animals." Herbart was followed by a school of brilliant disciples who excelled in sharp observations and wide-reaching deductions.

Thus both in England and Germany a scientific treatment of psychological questions was made possible by the victory of observation over speculation.

We must turn again to Galilei for the development of still another method, that of experiment. The step taken by Galilei was a simple but mighty one. It is difficult for us of the present day to understand the state of mind at the time of Galilei. The works of Aristotle were considered as the final authorities for all matters of fact, and no one ever dreamed of looking for facts except by reading books. Aristotle had said, among other things, that bodies fell at rates depending on their weight.

"Why he said so nobody knows. He cannot have tried. He was not above trying experiments, like his smaller disciples; but probably it never occurred to him to doubt the fact. It seems so natural that a heavy body should fall quicker than a light one, and perhaps he thought of a stone and a feather and was satisfied.

"Galilei, however, asserted that the weight did not matter a bit, that everything fell at the same rate (even a stone and a feather, but for the resistance of the air), and would reach the ground in the same time.

"And he was not content to be pooh-poohed and snubbed. He knew he was right, and he was determined to make every one see the facts as he saw them. So one morning before the assembled University, he ascended the famous leaning tower, taking with him a 100 lb. shot and a 1 lb. shot. He balanced them on the edge of the tower, and let them drop together. Together they fell, and together they struck the ground." [1]

This was the tocsin which sounded the approach of future armies of experimentalists who were to enter in succession so many domains of human knowledge.

The origin of a third scientific method, namely, that of measurement, is to be sought in antiquity. The ancient astronomers determined the intervals of a time for the heavenly bodies.

"By the introduction of the astrolabe, Ptolemy and the later Alexandrian astronomers could determine the places of the heavenly bodies within about ten minutes of arc. Little progress then ensued for thirteen centuries, until Tycho Brahé made the first great step towards accuracy, not only by employing better instruments, but even more by ceasing to regard an instrument as correct. Tycho, in fact, determined the errors of his instruments and corrected his observations. He also took notice of the effects of atmospheric refraction, and succeeded in attaining an accuracy often sixty times as great as that of Ptolemy. Yet Tycho and Hevelius often erred several minutes in the determination of a star's place, and it was a great achievement of Roemer and Flamsteed to reduce this error to seconds. Bradley, the modern Hipparchus, carried on the improvement, his errors in right ascension, according to Bessel, being under one second of time, and those of declination under four seconds of arc. In the present day the average error of a single observation is probably reduced to the half or quarter of what it was in Bradley's time; and further extreme accuracy is attained by the multiplication of observations and their skilful combination according to the theory of error." [2]

These errors of observation arise from surrounding conditions, from the apparatus, and from the observer

[1] Lodge, "Pioneers of Science," 90.
[2] Jevons, "Principles of Science," 271, London, 1887.

himself. It is part of the trade of a scientist that he must make a special study of these sources of error with a view to eliminating or controlling them. Thus on every hand there were men on the watch for their own peculiarities; what was more natural than that it should sometimes occur to them to study these peculiarities for themselves? This occasionally happened for special purposes, for amusement, &c. A large mass of miscellaneous pyschological material (chiefly in optics) had accumulated in this way, the particular value of it lying in the fact that it consisted of measurements. It was inevitable that at some time the man would come forward to develop a system of mental measurements.

Turning from measurements, we find still another method that has inevitably appeared in psychology, namely, scientific statistics. This method is, in its mathematical form, the gift of a series of men like Poisson, Bernoulli, and Laplace. Its application to human beings was accomplished by Quetelet. Fechner pointed out the median as an advantageous mean in statistical work, and left behind him an as yet unpublished but nearly completed work on the methods of statistics. Galton has used statistics for the anthropological treatment of mental qualities. In psychological research it has been used by Ebbinghaus and others. The possibility of the successful use of psychological measurements and statistics combined has been proven by researches in the Yale Laboratory.[1]

The final method to be considered is that of mathematical analysis. The great mathematicians have repeatedly run across psychological problems and solved them as far as possible without the basis of experimental

[1] See the explanation in Scripture, *Untersuchungen über die geistige Entwickelung der Schulkinder*, "Zt. f. Psychol. und Physiol. d. Sinn.," 1895, x. 161.

data. The law of relativity (p. 267) was first enunciated by Daniel Bernoulli in respect to the psychical value of physical possessions. Physical possessions have a value or a meaning to us only as the means of producing sensations in us. In this respect the gain of a guinea is a very different matter for a rich person and a poor one; to the poor man it will bring several days of happiness, while the rich one will not notice it. Generalising the statement, we can say that the value of any physical possession is relative to the amount we already have. Upon such considerations as these, Bernoulli developed formulas equivalent to the differential and logarithmic ones we have spoken of when considering Fechner's law (p. 271). Further developments were made by Laplace and Poisson, and the results were accepted by the later writers on the subject. To make the law a general psychological one, nothing more was needed than to bring it into relation with measurements of mind. It was inevitable that methods of measurement would be found, and the union of well-established facts with the methods of analysis would inevitably produce for the first time a perfected science of mind.

This survey of the methods that have been introduced into scientific research in general leads us to the conclusion that sooner or later the world would demand an investigation of the phenomena of mind according to such methods. The particular way of applying them on a large scale required the development of a fuller science of psychology than could be obtained by observation alone, and, with the forces of civilisation and science behind the demand, the new psychology was an inevitable event.

CHAPTER XXXII.

SOURCES OF THE NEW SCIENCE.

PSYCHOLOGY, as already explained, has been largely the product of other sciences. In most cases the first impulse to the investigation of psychological phenomena was given by the discovery of sources of error in the other sciences which were due to the scientist himself.

In astronomy Tycho Brahé did not accept his instruments as correct, but determined their errors; it was not, however, until centuries later that a suspicion arose concerning the possibility of errors in the observer.

Astronomers have to record the time of the passage of heavenly bodies across parallel lines in the telescope. When the star is about to make its transit the astronomer begins counting the beats of the clock. As the star approaches and passes the line he fixes in mind its place at the last beat before crossing and its place at the first beat after. The position of the line in respect to these two places gives the fraction of a second at which the transit occurred

In 1795 the British Astronomer Royal found that his assistant, working with another telescope at the same time, was making his records too late by half a second. Later on, this amounted to 0.8^{s}. This difference was large enough to seriously disturb the calculations, and as the astronomer did not suspect that he himself might

be wrong, the blame was laid on the assistant.[1] In 1820 Bessel[2] systematically compared his observations with those of another observer for the same star. They found a difference of half a second. Later he made similar experiments with Argelander and Struve with the result of always finding a personal difference.

Bessel sought for the cause of this "personal equation" by varying the conditions. He first made use of the sudden disappearance or reappearance of a star instead of steady motion. The personal difference was much decreased. This seemed to indicate that the trouble lay in comparing the steady progress of the star with the sudden beat of the clock. The next step was to change the beats, with the result that for Bessel the observations were made later with the clock beating half seconds than with one beating seconds whereas Argelander and Struve showed no particular change. One other point was investigated, namely, the effect of the apparent rate of the star; within wide limits the personal equation was not changed.

About 1838 the personal equation began to receive regular notice in astronomical observations, as appears from the publications of Airy[3] and Gerling[4] of that year.

It was natural to wish for a comparison of the astronomer's record with the real time of transit. At the suggestion of Gauss an artificial transit was arranged by Gerling, the object observed being a slow pendulum. This is probably the first measurement of a reaction time. In 1854 Prazmowski[5] suggested an apparatus carrying a luminous point for a star, and closing an elec-

[1] "Greenwich Astronomical Observations," 1795, iii. 319, 339.

[2] "Astronom. Beobacht. d Sternwarte zu Königsberg," VIII. iii, XI. iv., XVIII. iii.

[3] "Greenwich Astron. Observations," 1838, xiii.

[4] "Astron. Nachrichten," 1838, xv. 249.

[5] "Comptes rendus," 1854, xxxviii. 748.

tric circuit at the instant it passed the line; a comparison of the true time with the astronomer's record would give the real amount of his personal equation. From this time onward various forms of apparatus were invented and numerous investigations were carried out. The astronomers found that in such observations sometimes the star was seen to pass the line too soon, sometimes too late; and that it depended upon every variation in the method of observing and in the mental condition of the observer.[1]

Let us turn for a moment to another science. The new physiology, begun by the pupils of Johannes Müller, in which the phenomena of life were to be explained by physical and chemical processes, had undergone a remarkable development. Du Bois-Reymond had taught how to apply the experimental methods and apparatus of physics to the study of physiological processes. Soon after this, Helmholtz measured the velocity of nervous transmission (1850), an experiment that Johannes Müller had considered hopeless. This involved the construction of the myograph and the application of Pouillet's method of measuring small intervals of time.

The nerves, however, are only the peripheral portions of the nervous system; the desire lay near to measure the time occupied by the brain processes. Such measurements have been down to the present day impossible by direct physiological methods. It was, however, a sufficiently settled fact that the brain processes are closely accompanied by mental processes. This consideration led to the employment of similar methods on living human beings. The stimulus was

[1] The history of the personal equation given above is summarised from Sanford, *Personal Equation*, "Am. Jour. Psych.," 1888, ii. 3, 271, 403.

applied to the skin, to the eye or to the ear and the time acquired for the subject to respond by a muscular movement was measured. Since the subject could tell what he experienced under different variations of the experiment it was found possible to measure the time of sensation, of action, &c. The physiological processes corresponding to these mental processes were to some extent known. It was soon discovered, however, that other mental processes, *e.g.*, discrimination, association, &c., could be introduced in such a way as to be measured.

In 1865 Donders began to make a systematic attack on the problem of psychological time-measurements and was soon followed by Exner. Helmholtz had already directed the experiments of his pupil Exner in measuring the time of sensation (p. 93); in 1877 the work of Auerbach and v. Kries on mental time issued from his Berlin laboratory.

The interest of the physiologist lies, however, mainly in the deductions to be drawn concerning brain action. Even from the simpler forms of reaction-time the amount of physiological knowledge to be obtained is small, and for the more complicated forms it is zero. It was natural, therefore, for physiologists to pursue the subject but little further.[1]

Thus the two sciences of astronomy and physiology discovered and developed the methods of investigating mental times; the further development was the task of psychology. The result has been sketched in outline in Part II.

[1] For the historical account of experiments on reaction-time see Buccola, "La legge del tempo nei fenomeni del pensiero," Milano, 1883, and Ribot, "La psychologie allemande contemporaine," Paris, 1885; for a summary with literature, see Jastrow, "Time Relations of Mental Phenomena," New York, 1890.

Another source of the new psychology is to be found in the physiological study of the sense organs. The most obvious method for determining the functions of the nerves and end organs of the skin, the nose, the ear, or the eye, is to ask the living subject what he feels when various stimuli are applied. In this way there has arisen a large body of knowledge concerning the sensory functions of the nervous system. In this form, however, the problem is a purely psychological one. To inquire if the skin "feels" heat is from a physiological point of view an indirect question. Physiologically the nerves of the skin may respond to heat by some chemical process. That they do so respond may be inferred on the hypothesis of a correspondence between the occurrence of a sensation of heat and the action of the nerve.[1] The direct question is one of psychology ; it is asked by physiology for its own purposes, and the psychological data are collected as long as they are of use in this way. Physiology, however, is "physics and chemistry of the body," and as soon as psychological data cease to afford physical deductions the interest of the physiologist generally ceases. The study of the psychology of sensation and action, however, has formed and still forms an important portion of physiology.

Historically considered, the study of the sensations of the skin received its first great impulse from Ernst Heinrich Weber's monograph : "Tastsinn und Gemeingefühle."[2] This has been followed by the work of a host of investigators trained in the laboratories of Ludwig, Du Bois-Reymond, and their pupils.[3]

[1] I am, of course, not speaking of purely objective physiological experiments on nerves.

[2] Wagner's "Handwörterbuch d. Physiologie," 1851, iii. (2), 561 ; also separate. An account of Weber's life is to be found in Ludwig, "Rede zum Gedächtniss an Ernst Heinrich Weber," Leipzig, 1878.

[3] For summaries and references, Funke und Hering, "Physio-

The physiology of the eye likewise originated much of the psychology of sight. Concerning the functions of the optical system physiology can scarcely be said to have gone beyond the dioptrics of the eye. Nearly all further knowledge consists of deductions from the mental experiences of the subject. For example, physiology knows almost nothing concerning the functions of the retina. Psychologically, however, the colour sensations and their combinations can be accurately measured. It is true that the investigations of colour vision have been and are mainly carried out by physiologists and physicists ; but the point of view has become primarily a purely psychological one. This is strikingly exemplified in the researches of König (p. 333) from which physiological deductions are practically excluded. For the various other phenomena such as those of the optical illusions, of monocular and of binocular space we have at present no hope of anything beyond a psychological knowledge ; and the investigations of Hering,[1] Helmholtz, and others can be regarded as direct contributions to psychology.

There is a third science whose influence is to-day the strongest of all. Physics is theoretically the co-ordinate science to psychology. Every direct experience has an objective, or physical, and a subjective, or psychological, side. Again, the fundamental science of nature is physics, that of mind is psychology. Practically

logie der Hautempfindungen und der Gemeingefühle ; " Hermann' " Handbuch der Physiologie," 1880, iii. (2), 287 ; and Beaunis, "Nouveaux éléments de physiologie humaine," ii., Paris, 1888.

[1] Among the notable publications of Ewald Hering (formerl Professor of Physiology in Prag, now in Leipzig) special mentio is due to *Der Raumsinn und die Bewegungen des Auges*, i Hermann's "Handbuch der Physiologie," iii., Leipzig, 1879. A list of his works can be found in the bibliography appended to th last edition of Helmholtz's " Physiologische Optik."

however, psychology receives from the most powerful science of modern times an invaluable protection, and an uninterrupted series of scientific gifts. The photometry of Lambert led not only to the methods of modern technical photometry, but also to the measurement of our sensations of light. The law of relativity had been, before Fechner's time, established for light by Bouguer, Masson, Arago, Herschel, and Steinheil. The study of the errors of observation in physics and astronomy has led not only to the science of physical measurements, but also to that of psychological measurements. Newton, Young, and Maxwell, began not only the science of ether vibrations, but also the science of sensations of light. The laws of mechanics apply not only to inanimate objects, but also to the results of our own volitions. In fact, in every department of experimental psychology progress has been and still is closely dependent on the achievements of physics and technology.

Another powerful influence in forming the new psychology was "physiological psychology." Alkmaion (about 520 B.C.) seems to have been the first to carefully consider the functions of the nervous system in regard to mental life. In modern times the earliest prominent figure appears to have been Varoli (d. 1575), who considered the cerebrum to be the organ of mind. Gall (d. 1828) went further in specialising the convolutions for this purpose, and in investigating the systems of fibres. The doctrine of Flourens (1842), the discoveries of Broca (1863), the experiments of Fritsch and Hitzig (1870) and of Ferrier (1873), the extirpations by Munk and Goltz, the development studies by Flechsig and the clinical observations of Charcot, have led to a remarkable activity in the study of the functions of the brain. The ultimate aim is a

thoroughly scientific knowledge of the action of the brain and of its connection with mental life in health and disease. The direct knowledge of mental life assumed in these researches is that common to everybody; psychological science is generally disregarded or distrusted.

In Germany the physiological data first received recognition in connection with psychology through the work of Lotze ("Medicinische Psychologie," 1852). The work has had its chief influence in introducing empirical methods of thought, and in thus preparing for experimental psychology.

A similar movement was begun somewhat later in England. Dissatisfied with the condition of introspective psychology, and stimulated by the achievements of brain physiology, various enterprising minds undertook the reconstruction of psychology on the basis of physiology. The vigorous work of men like Maudsley ("Physiology and Pathology of Mind," 1867, and later editions) brought about closer relations between academic psychology and the phenomena of pathology, of animal life, &c. The principle of this "physiology of mind" (denying the validity of introspection and hoping to determine mental facts by brain-dissections) was a false one, as has been explained in Chapter I.; yet introspective psychology began to take a wider view, and in the works of Carpenter ("Mental Physiology," 1874) and most of the later psychologists we find an introspective account of mind preceded and accompanied by accounts of the nervous system. Finally, the whole movement toward the introduction of "natural science methods" into psychology has been greatly favoured by the exponents of evolution, such as Spencer ("Principles of Psychology," 1855; 3rd edit., 1880).

This tendency of physiological psychology has of

late developed in two directions. On the one hand, it has led directly to the establishment of experimental psychology, which in some places still goes by the older name, although its standpoint has become a purely psychological one. On the other hand, it has led to a psychological program which at the present day is far from successful fulfilment. The "future" psychology was to begin with first investigating the nervous system, and then deducing psychological laws therefrom. The brain was to be accurately mapped out into faculties, the paths of nervous currents were to be traced along various fibres, and the interaction of nervous molecules was to be known in every particular; it was even expected by some that various cells could be cut out with a memory or a volition snugly enclosed in each. In other words, there was to be no psychology except on the basis of a fully developed brain-physiology. Unfortunately very little has been ascertainable concerning the finer functions of the nervous system, and aside from a general knowledge that the cerebellum has to do with co-ordination of movements, the convolutions of Broca have to do with speech, and similar facts, nothing of even the remotest psychological bearing has been discovered concerning the functions of the brain. The roseate hopes of those who expected a new psychology out of a brain-physiology were totally disappointed. In the effort for something new, however, the psychologist supplied the data concerning the "molecular movements" in the brain by his own imagination; the familiar facts of mind were retold in a metaphorical language of "nerve currents," "chemical transformation," &c., of which not one particle had a foundation in fact.[1]

[1] It is to be noted that these statements refer to investigations of and speculations on the brain for *psychological* purposes. For

While this was going on, physics had through Helmholtz,[1] Mach,[2] and others gradually come to a clear understanding of the relation of its facts to the immediate facts of consciousness. Direct experience as present in our sensations was accepted as supplying the facts of physics. For example, in measuring the length of a bar a visual sensation, the unit of measurement, was applied to another visual sensation, the bar. Indeed, as was clearly recognised, every direct measurement of physics was primarily a comparison between sensations—in other words, a psychological measurement. From this combined measurement the physicist reduced as much as possible the psychological elements; it was but a step for the psychologist to reduce the

physiological purposes the case is utterly different. The development of brain anatomy and of the knowledge concerning the localisation of cerebral functions is among the greatest achievements of modern times. (For a historical sketch and an account of the latest remarkable discovery see Flechsig, "Gehirn und Seele," Leipzig, 1896.) Moreover, the collection of facts and the development of theories of the nervous activities accompanying mental phenomena have given rise to the science of physiological psychology. (As a representative work see Exner, "Entwurf zu einer physiologischen Erklärung der psychischer Erscheinungen," I. Theil, Leipzig, 1894; for a convenient summary, see Ziehen, "Leitfaden der physiologischen Psychologie," 2nd ed., 1893, also translated.) With these sciences, however, the psychologist has very little to do. The study of brain function has contributed comparatively little to our knowledge of mental life except in abnormal conditions; the deductions of physiological psychology concerning nervous function are based upon the facts of experimental and observational psychology, and are still so unsettled as not to allow additional deductions backward.

[1] Helmholtz, *Ueber das Ziel und die Fortschritte der Naturwissenschaft*, "Populäre wiss. Vorträge," Braunschweig, 1871. Helmholtz, "Die Thatsachen in der Wahrnehmung," Leipzig, 1879.

[2] Mach, "Die Mechanik in ihrer Entwickelung," Leipzig, 1883, 2nd ed., 1889; also translated into English, Chicago, 1895 (Mach's earlier monographs are mentioned in the preface). Mach, "Beiträge zur Analyse der Empfindungen," 141, Jena, 1886 (translated).

physical elements in order to have a psychological measurement.[1]

This step made psychology for the first time a quantitative science in the full meaning of the term with all the previous achievements of physics for its use.

There is still another science which we must consider, namely, the old psychology. By the "old psychology" I mean psychology before the introduction of experiment and measurement.

In the previous chapter it was noticed that the victory of observation over speculation in mental science was mainly due to Hobbes and his successors. Thomas Hobbes (1588–1679), educated at Oxford and Paris, personally acquainted with Bacon, and deeply impressed with the importance of the Galileian and Baconian methods, was the founder of an observational —as opposed to a speculative and dogmatic—science of mind.

After Hobbes the empirical method made little progress till the time of Locke (1632–1704). Educated at Oxford, and specially inclined to the natural sciences and medicine, Locke proposed to analyse the phenomena of the human mind directly on the basis of the facts themselves, expressly excluding all questions of metaphysics and of physiological psychology.[2]

[1] The psychological standpoint has been clearly stated by Wundt, *Ueber die Messungen psychischer Vorgänge*, "Philos. Studien," 1883, i. 1; *Weitere Bemerkungen über psychische Messungen*, same, 463; *Ueber die Eintheilung der Wissenschaften*, "Philos. Studien," 1889, v. 1; *Ueber die Definition der Psychologie*, "Philos. Studien," 1896, vii. 1; *Ueber naiven und kritischen Realismus*, "Philos. Studien," 1896, xii. 307. I have taken the same view in *The Problem of Psychology*, "Mind," 1891, xvi. 305; *Psychological Measurements*, "Philos. Review," 1893, ii. 677.

[2] Locke, "An Essay concerning the Human Understanding," bk. i. ch. i. sect. 2.

Locke's introspective standpoint received strong emphasis and thorough elaboration in the works of Berkeley (1685–1753). Somewhat later the empirical method, which Locke did not succeed in fully carrying through, was applied in a thoroughly consistent fashion by Hume (1711–1776). Thus at this early date the problem of psychology and its fundamental method were definitely understood and clearly expressed.

The further development of psychology by Hamilton, Reid, Mill, and the later writers belongs to the history of "general" or "descriptive" psychology rather than to an account of that part of the larger science with which we are at present concerned. It is, however, not easy to draw the line between the new and the old; some of the important works, written before the recognised introduction of laboratory methods but yet using the results of experimental work as far as obtainable, will be noticed in the following chapter.

The method of direct observation of mental life is the only possible one, and until it had received a firm basis any science of psychology was impossible. As has been explained in Part I., all the other methods of psychology are only refinements of this method. The new psychology is thus merely a development on the basis of the old; there is no difference in its material, no change in its point of view, and no degeneration in its aims. What the old tried to do, namely, to establish a science of mind, and what it did do, as far as its means allowed, the new psychology with vastly improved methods and facilities is striving to develop in finer detail.[1]

[1] For historical accounts, see also Külpe, "Grundriss der Psychologie," 24, Leipzig, 1893; Baldwin, *Psychology Past and Present*, "Psych. Rev.," 1894, i. 363.

CHAPTER XXXIII.

FECHNER, HELMHOLTZ, WUNDT.

THE existence of experimental psychology as an independent science, begins with Fechner.[1]

Gustav Theodor Fechner was born, 1801, in Grosssaerchen (Niederlausitz), Prussia. As a student of medicine at the University of Leipzig, he was a pupil of Ernst Heinrich Weber. His first scientific inspiration came from Oken's "Natural Philosophy." He was gradually led to more exact reasoning through the textbook of Biot, which turned him to the study of physics. He translated several French works and produced a number of original contributions. At the same time, under the name of Dr. Mises, he published a number of humorous and satirical essays. Chiefly on account of his scientific investigations, he was made professor of physics, Leipzig, 1834, but he was obliged to retire in 1840 on account of illness. His interest had long been vividly fixed on the relations of mind and body; his training in physics made him desire some method of measuring sensation and some law of its connection with the physical processes in the nervous system.[2] Seeking to express

[1] For the details of Fechner's life, see Kuntze, "Gustav Theodor Fechner," Leipzig, 1892; for a brief account of his life and work, see Lasswitz, "G. Th. Fechner," Stuttgart, 1896.

[2] Fechner gives the account himself in his "Elemente der Psychophysik," last section.

the relation between mental and bodily processes, Fechner had thought of a law of direct proportionality; for more complicated processes he had proposed the relation between lower and higher arithmetical series, or even geometrical ones. These were merely schematic relations; the unit of measurement was lacking until it

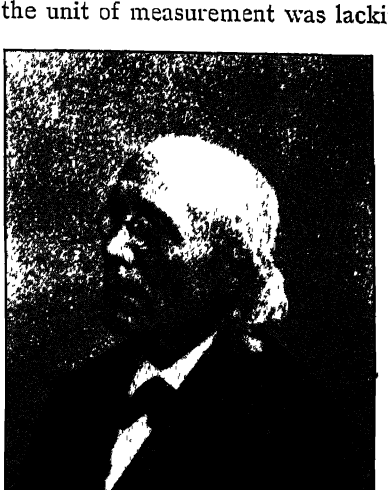


Fig. 118. GUSTAV THEODOR FECHNER.

occurred to Fechner to use the relative increase of physical energy (or $\frac{dR}{R}$, where R denotes the physical energy) as the measure of the increase in the corresponding psychological intensity. The fundamental formula followed at once as a deduction (p. 271). The confirmation of this law appeared to be found in the ordinary

experience that the increase in sensations of light did not keep pace with the physical increase, a fact that was moreover true of all our sensations. Upon sending an account of his idea to the physicist, W. Weber, he received the reply that, though the idea was a good one, it would not find acceptance until it was established by concrete examples. After long seeking, Fechner devised some special experiments in light.

"With a half-covered sky it is generally easy to find a pair of neighbouring shades in the clouds which exhibit only the trace of a difference to the eye. . . . After I had found two such components of a just perceptible difference, I placed two grey glasses before the eyes. . . . One of these let through a little more than one-third of the light, and both together at the most one-seventh of it. . . . It was natural to suppose that the difference which was before only just perceptible, would be lost by such a reduction or would at least become markedly less clear. This was not the case. The difference remained at least as clear as before." [1]

The just perceptible difference, therefore, was a certain fraction of the stimulus regardless of the absolute intensity. This is what was demanded by Fechner's law. Thereafter Fechner undertook experiments with lifted weights (p. 268), which he carried on for many years; whereupon he found that—as he relates—what he sought and had gained by hard labour was already present to a certain degree in the clear statements and experimental data of Ernst Heinrich Weber (p. 267). The investigations of Volkmann, the previous work of Masson, Steinheil's measurements of the stars—these and other data were all gathered by Fechner in support of his law.

Still later, Fechner developed his method of right and wrong cases (p. 268), wherefrom he was led to a consideration of the so-called method of average errors. To Fechner's mind, the law of the relation between the

[1] Fechner, "Elemente der Psychophysik," i. 141.

psychical and physical worlds and the unit of measurement were found.[1]

The work of Fechner was followed by many discussions. The chief criticism to which it was subjected was that of G. E. Müller, who, in his work "Zur Grundlegung der Psychophysik," Berlin, 1878, gave the subject a most thorough examination from every point of view. The position of this work in the history of psychology was stated by Fechner himself, who says that nothing had done more to render the foundation of psychophysics firmer.

In the previous chapter we found that the name of Helmholtz occurred in connection with many of the sources of psychology.

Hermann von Helmholtz was born in 1821 at Potsdam, Prussia.[2] He prepared himself for the profession of army surgeon, and after passing through a practical course in the Charité Hospital, in Berlin, took up his duties in Potsdam. Happening to succeed in getting a microscope, he at once used it for scientific investigation, obtaining as a result the doctor's degree in 1842 by a valuable thesis on the ganglia of the invertebrates. In 1847 he published his essay "Ueber die Erhaltung der Kraft." This essay, the most important that had appeared for centuries in the history of the natural sciences, was his first one in physics. Freed from military service through the interposition of Alexander von Humboldt, he was appointed instructor in anatomy in the Berlin Art Academy (1848). He

[1] The list of Fechner's works is to be found at the end of the first volume of the second edition of his "Elemente der Psychophysik," Leipzig, 1889; and also in Kuntze, "Gustav Theodor Fechner," Leipzig, 1892.

[2] In the following account I have relied mainly on a tribute by Wiedemann, "Annalen der Physik und Chemie," 1895, liv. i.; see also v. Bezold, "Hermann von Helmholtz," Leipzig, 1895.

was then called (1849) to Königsberg as professor of physiology and pathological anatomy, and to Bonn (1855) as professor of anatomy and physiology. In the meantime physiology had become an independent science through the work of Ernst Heinrich Weber, Ludwig, Du Bois-Reymond, Brücke, and himself. A

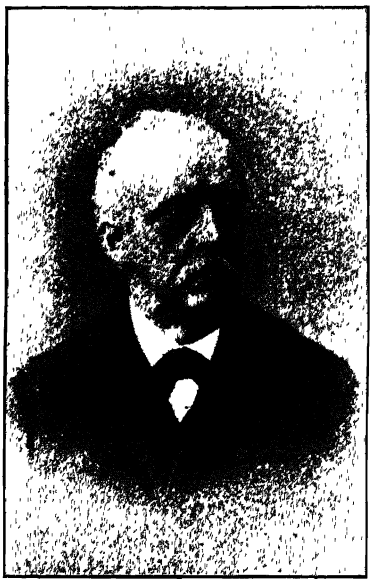


Fig. 119. HERMANN VON HELMHOLTZ.

special chair for physiology alone was founded and given to him at Heidelberg (1858).

Helmholtz's first great achievement in physiology was the measurement of the velocity of nervous impulses (p. 444), in 1850. He then turned to the investigation of the eye and the ear. For investigating the eye it was first necessary to study

more accurately the anatomical and geometrical conditions of its movements and mechanical changes. For this purpose he invented the ophthalmometer. This was first used for measuring the curvature of the cornea. In the course of his further investigations he invented the ophthalmoscope for directly viewing the interior of the eye. He then proceeded to the psychology and physiology of vision. The results of his optical work are to be found mainly in his "Physiologische Optik," which appeared in sections during the years 1856–66. His psychological work led him to philosophical considerations and to mathematical developments concerning space, number, and measurement. Even while studying the eye he produced his "Lehre von den Tonempfindungen" (1862).

By the establishment of the principle of the conservation of energy, Helmholtz had become the foremost physicist of Germany. In 1871 he was called to the Professorship of Physics in Berlin. From this time onward he stimulated many psychological investigations, although he made none himself. Upon taking up the work (1884) of a second edition of the "Physiologische Optik" he again applied his powers of analysis to psychological problems with the usual result. In 1888 he accepted the care of the Physikalisch-Technische Reichsanstalt, in which the chief work was the foundation of the methodology for measurements of the highest accuracy. He died in 1894.

"When we view the scientific life of Helmholtz we are amazed at the extent and the fulness of his achievements. While many other prominent investigators select single themes, which they work out with the best of results, with Helmholtz all the undertakings of his life fit harmoniously together. From the investigation of the sense organs, in which definite physical and physiological data were opposed to arbitrary suppositions concerning the action of a vital

force, the same fundamental thought continues down to the 'Erhaltung der Kraft,' in which the changes of energy were limited by its constancy and indestructibility. Almost all his later achievements are based on the same thought, and thus Helmholtz passes gradually, in correct scientific developments, from general mechanical considerations to hypotheses concerning the structure and inner nature of matter, with which problem his life closes." [1]

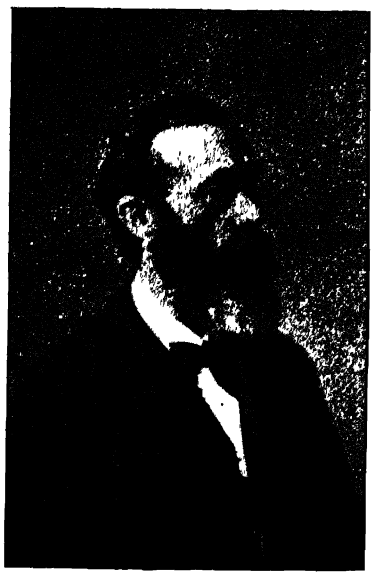


Fig. 120. WILHELM WUNDT.

The third great figure in the history of experimental psychology is Wundt.

Wilhelm Wundt was born at Neckerau in Baden in 1832. Having studied medicine at Tübingen, Berlin, and Heidelberg, his academical career began with a place as instructor in physiology at Heidelberg. In

[1] Wiedemann, as before.

1862 he published his "Beiträge zur Theorie der Sinneswahrnehmung," containing important investigations in the psychology of the senses. This was followed (1863) by "Vorlesungen über die Menschen und Tierseele" (second edition 1892, also translated). In 1864 he was made assistant professor of physiology; in 1874 he was called to Zurich as professor of philosophy, and in 1875 to Leipzig. Several earlier works were connected with physics and physiology.

In 1874 there appeared his great psychological work "Grundzüge der physiologischen Psychologie" (fourth edition, 1893). In 1883 begins the first volume of his "Philosophiche Studien." His latest psychological book, "Grundriss der Psychologie," appeared in 1896 (also translated). The development of psychology led him to the study of philosophical problems in methodology, epistemology, and metaphysics; this resulted in his "Logik," "Ethik," and "System der Philosophie."

Aside from his position as a philosopher, with which we have here nothing to do, probably the most important and far-reaching achievement of Wundt's life is the successful establishment of the first psychological laboratory. Upon his arrival at Liepzig (1875) one room was given him for setting up his own apparatus and conducting his investigations. The first students to appear were two who entered in the winter of 1879–80; one of these was G. Stanley Hall.

In 1886 the laboratory was recognised as an official university institute, and an assistant was appointed. The internal history of the laboratory is to be found in the series of monographs describing investigations made in it, which are contained in the twelve volumes of the "Philosophische Studien." Among the names to be found there are those of Kraepelin, Merkel, Cattell,

Lehmann, Kuelpe, Kirschmann, Martius, v. Tschisch, Witmer, Pace, Titchener, Marbe, Meumann, Kiesow, Judd, Stratton, and others, who now have laboratories of their own or have made valuable contributions to science.

CHAPTER XXXIV.

THE PRESENT SITUATION.

At the present day the new psychology is a recognised member of the family of the sciences; its condition, however, and the character of its progress are different in the various countries.

Down to a few years ago, experimental psychology was almost exclusively a German science. I have already traced its course in the preceding chapters.[1] At the present day there are laboratories in connection with the professorship of philosophy in several of the larger universities, *e.g.*, Berlin, Bonn, Göttingen, Würzburg, &c., and also one at Heidelberg in connection with the chair of psychiatry.

In the year 1890 a special periodical for psychology, the "Zeitschrift für Psychologie und Physiologie der Sinnesorgane," was established by Ebbinghaus and König, with the co-operation of Aubert, Exner, Helmholtz, Hering, v. Kries, Lipps, G. E. Mueller, Preyer, Stumpf, and Pelman, all of whom have contributed scientific material of the highest value.

Germany's position in psychology at the present day has been made so plain in previous chapters of this book, that further explanation or comment on the character of

[1] For an account of German psychology to 1885 see Ribot, "La psychologie allemande contemporaine," Paris 1885 (also translated).

the work would be superfluous; I will add merely a few words concerning the prospects for the future.

In spite of the fact that so many psychological investigations still come from Germany, the number of laboratories is not large, and the equipment is often not extensive. The progress, however, though slow, is sure. The most significant sign lies in the fact that the Prussian Government aims to found laboratories wherever the appropriate persons are to be found. "To be sure, there is still somewhat of a lack of the appropriate ones. Among the great number of teachers of philosophy in the German universities there are still relatively few who devote themselves specially to experimental psychology."

Turning to a consideration of the neighbouring country, France, let us place the pen in the hand of the best-known representative of the new psychology, A. Binet.[1]

"The first work of experimental psychology which we possessed in France is the book of Taine on 'Intelligence' (1870). This book, which created an epoch among us, is above all a treatise on pathological psychology, built on observations taken from the alienists. It is a very systematic work; many of its parts are now antiquated; others, like the chapters on representations, are still of value.

"A second important date, 1875, is that of the appearance of the 'Revue Philosophique,' founded and directed by Th. Ribot. Professor Ribot has contributed greatly to the development of psychology

[1] Professor Binet was kind enough to prepare the account for this book. I venture to hope that it will be of special interest in view of the fact that very little is known to the outside world concerning the history of psychology in France. The term "experimental" has in French a slightly wider meaning than in English.

in France: in the first place, by the 'Revue' which he has generously opened to new ideas; again by his works ('L'Hérédité psychologique,' 'Psychologie anglaise contemporaine,' 'Psychologie allemande contemporaine,' 'Maladies de la mémoire,' 'Maladies de la volonté,' 'Maladies de la personalité,' 'Psychologie de l'attention,' &c.),[1] in which he has condensed in a remarkable way a profound understanding of physiological psychology; and, finally, by his instruction at the Collége de France. We thus consider him in general as our leader.

"In 1878 Professor Charcot, the eminent physician of the Salpêtrière, began to publish researches on hypnotism, a nervous condition still poorly defined, which the French psychologists have studied with predilection. At a later date Professor Charcot, on the occasion of clinical instruction concerning aphasia, presented an ingenious theory of language and thought (the theory of visual, auditory, and motor images) which has become classical, although it is probably incorrect in many fundamental points. The same is true for his theories of hypnotism, some of which have fallen to pieces—a fact which does not hinder his investigations from having rendered great services to science.

"After these three names—the most important of all—we must mention Charles Richet, professor of physiology in the faculty of medicine, who has made numerous studies on pain, somnambulism, telepathy, &c. ('L'Homme et l'intelligence,' 'Psychologie générale,' &c.); Charles Féré, physician at the Bicêtre hospital, whose name is connected with questions of heredity and of the relation between stimulation and movement ('Sensation et mouvement,' 'Pathologie des emotions,' 'Le magnetisme animal'); Beaunis, who with Bernheim has aided in showing the *rôle* of suggestion in hypnotism

[1] Nearly all of these have been translated into English.

('Le somnambulisme provoqué'); Pierre Janet, who, by his studies on hypnotism, has especially thrown light on the phenomena of dissociation of consciousness ('L'automatisme psychologique'); and Bourdon, known for his conscientious studies on memory and association of ideas ('L'expression des emotions et des tendances dans le langage'). We must mention also Sollier, Seglas, Dumas, Godfernaux who, from various points of view, have contributed to the psychology of alienism (I, of course, do not speak here of the alienists by profession); Jules Soury, who has devoted himself specially to bibliographical psychology and has published, in this sense, valuable works on the brain; Jacques Passy, to whom we owe well-finished and precise researches on the psychology of odours; and Perez, the observer of children. Of descriptive writers, of critics, and of theorists like Arréat, Egger, Liard, Paulhan, Souriau, Séailles, Tarde, Le Bon, and, above all, Fouillée, and of the philosophers, the names are too numerous for complete citation; we must, however, mention the greatest of them, Renouvier, the celebrated neo-Kantian.

"We see by these names that the principal efforts of French psychologists have been directed toward pathology; the chief subjects of study have been the insane and the hysterical. Let us note, moreover, that the majority of the French psychologists are physicians and not psychological specialists; from this it results that the development of the science has occurred outside of the laboratory, away from official guidance and disregarded by the university.

"It is only eight years ago that the board of superior instruction created a chair of experimental psychology at the Collége de France, and about the same time (1889) founded the psychological laboratory of the Hautes Études, whose head director has been Professor

Beaunis. These are the two official institutions for psychology. They have not, at the present, the complete practical influence that one could have hoped for, because experimental psychology does not figure in any examination, and in France leads to nothing. Moreover, we do not have in our laboratory, to the same degree as in Germany, students who undertake investigations in order to obtain a diploma; students come to us only when they have a veritable vocation for psychology, and these are always rare. Among my students and assistants I will mention MM. Philippe and Courtier, M. Milhaud, who has been associated with me in most of my investigations, and M. Victor Henri who has already become known through his studies on memory and on tactile sensibility. Connected with our laboratory is the 'Année Psychologique,' founded two years ago, and edited by Professor Beaunis and myself.

"Finally, to be complete, I will mention the ephemeral existence of the 'Société de psychologie,' which was founded a dozen years ago under the auspices of Professor Charcot, and which has disappeared at the present time, from causes that are still discussed; I believe that its death arose from the fact that it allowed itself to be invaded by the spiritists and telepaths.

"We must now attempt to outline in a few words the character of the contributions which French scientists have made to experimental psychology. In this respect it is necessary to state, without bitterness but frankly, that our work is completely unknown abroad. Thus we see chiefly German works cited; ours are generally absent from the best-made bibliographies. Thus it comes about that the American students, who to-day form incontestably the majority of the students of psy-

chology and of the future psychologists, are educated in the German laboratories.

"The investigations in experimental psychology in France fall into two categories which form to a certain degree two successive periods.

"The first period is that of hysteria and hypnotism. There was a certain epoch, approximately from 1880 to 1890, during which there appeared in France a considerable number of works on the questions of hypnotism and of hysteria. The contributions came mainly from the school of the Salpêtrière and also from a rival school, that of Nancy. In spite of the objections made to these contributions, it is certain that they have taught us many things concerning susceptibility, the influence of excitations in relation to movements, hallucinations, and the phenomena of the division of consciousness. The danger of studies in hypnotism is that it has furnished an easy transition to the study of telepathy, clairvoyance, and other phenomena *ejusdem farinæ*, which have in France attracted a great number of mystics and amateurs of the semi-obscure.

"The second of the two periods which I have indicated is signalised by a relaxation of the studies in hypnotism. The effort is turned by preference to other questions—aphasia, arithmetical prodigies, memory, the superior intellectual functions, and also the organic and motor functions connected with intellectual states. I believe, as far as predictions go, that French psychology will long continue in this path, on which it entered about 1890. To this period belongs Bourdon, who has just established a small laboratory at Rennes (the second one in France). As is known, the contributions of this scientist are nearly all devoted to the phenomena of ideation among adults. The sche-

dules of questions and the investigations of Ribot on memory and abstraction belong also to this period. Finally, I may say that most of the investigations of this period have come from the Paris laboratory, which presents a special equipment for the study of organic and motor reactions, and for all that relates to the graphic method." [1]

In Italy there is one psychological laboratory, at Rome, in connection with the Anthropological Institute; it is conducted by Professor Sergi, who has published several works on psychology. A great deal has, however, been done in physiological laboratories, psychiatrical clinics, and asylums. Mosso (Turin) has made researches on fatigue (see Chap. XVI.), on the emotions and their effects, &c. Lombroso (Turin) has performed psychological experiments on criminals. Tamburini (Modena) has founded an institute for psychological work as a propædeutic to psychiatry. Morselli (Genoa) has made several psychological investigations. Tito Vignoli (Milan) has published valuable works on comparative psychology, &c.

Of the other European countries, Russia (Moscow, Dr. Tokarsky) and Switzerland (Geneva, Professor Flournoy) are the only ones that have laboratories with psychological specialists.

In England one of the most prominent figures is Alexander Bain, for many years Professor in the University of Aberdeen, whose comprehensive work,

[1] Professor A. Binet, director of the psychological laboratory of the Sorbonne (Hautes Études), has published numerous books and monographs; among them are: "La psychologie du raisonnement," "Le magnétisme animale," 'Les alterations de la personalité," "La vie psychiques des micro-organismes," "La psychologie des grands calculateurs et joueurs d'échecs," &c. (The historical part of the last work is based on my monograph, *Arithmetical Prodigies*, "Amer. Jour. Psych.," 1891, iv. 1.)

"The Senses and the Intellect" (1855, four later editions), has had a marked influence on the development of psychology. The standpoint is that of introspection with full recognition of physiological and experimental data. The results of experimental work have been carefully used at the appropriate places in several works on psychology by James Sully, Professor in University College, London ("Outlines of Psychology," "Human Mind," &c.). Professor George Croom Robertson, the former editor of "Mind," deserves special mention for his friendly attitude toward the new science. The brilliant work of Francis Galton ("Inquiries into Human Faculty," "Hereditary Genius," &c.) has had a lasting influence on many departments of experimental work. The psychological data concerning the sexes have been treated by Havelock Ellis ("Man and Woman"). The English quarterly, "Mind" (G. F. Stout), occasionally contains experimental articles, as also do the "Journal of Mental Science" and "Brain." An anthropometrical laboratory with considerable psychological apparatus was established by Mr. Francis Galton at South Kensington many years ago, and maintained at his own cost. In 1895, having obtained as many facts as he could utilise, Mr. Galton closed the laboratory and presented the apparatus to the anatomical department of the museum at Oxford, where some work was initiated. At Cambridge Professor Ward first lectured on experimental psychology in 1882. Practical teaching began in 1893, and is carried on by Dr. W. H. R. Rivers, who has two rooms at his disposal in the Physiological Laboratory. Four courses are given in the year—two in experimental psychology, and two in the physiology of the sense-organs—and there is a fair amount of apparatus (most complete as regards the study of vision) with which work can be done in most

departments of experimental psychology.[1] At present there is no psychological laboratory in London, but funds are now being collected for the establishment of such a laboratory at University College, and the matter is in the hands of an organising committee, consisting of Mr. Galton, Dr. Rivers, Professors Carey Foster, Karl Pearson, Schäfer, and Sully.

The University of Toronto has had a laboratory for a number of years (Professor Baldwin, then Dr. Kirschmann).

In the United States the new movement needed the endorsement of the older universities. This came from courses in physiological psychology by Professor James at Harvard and Professor Ladd at Yale. A decisive form was given to the movement by the publication of Ladd's "Elements of Physiological Psychology" (1887). This received immediate recognition; for a long time it was the only accessible source of information concerning the new psychology, and its character profoundly influenced the development of American psychology. James had already begun to collect material for laboratory work, but the first officially recognised laboratory was founded (1883) at Johns Hopkins University (Baltimore) by G. Stanley Hall, a pupil of Wundt. In 1887 the publication of the "American Journal of Psychology" (G. Stanley Hall) was begun; in 1888 the first professorship of experimental psychology was founded at Philadelphia (Cattell); in 1893 the "Psychological Review" (Cattell and Baldwin) was established; and in the same year a third psychological publication, "Studies from the Yale Psychological Laboratory," was begun. In 1890 James published his "Principles of Psychology," which aroused general public interest;

[1] Mr. Galton and Dr. Rivers have kindly supplied the information regarding their laboratories.

and in 1895 Sanford published the first part of his instructions for making elementary experiments ("Laboratory Course in Experimental Psychology"). In response to a demand for a book adapted to the average unscientific reader, "Thinking, Feeling, Doing" appeared in 1895.

The rapid development of experimental psychology in America has brought into existence more than twenty laboratories of all sizes and degrees of equipment, often quite elaborate.[1] During the last few years the smaller laboratories have multiplied so rapidly in the colleges and normal schools that it is impossible to give any account of them. With a few exceptions, every university or college of note has an equipment of some kind.

There are some peculiar characteristics of American psychology that have arisen from its rapid development. The invention of new and more accurate apparatus has come to the foreground through the establishment of the machine-shop with a trained mechanic as a fundamental part of the laboratory. This has long been recognised as the most profitable course in the great European and American laboratories of physics and physiology; in psychology it was first accomplished at Yale. Still another characteristic is the development of systematised courses of instruction on a large and carefully adjusted scale. This also is but the logical outcome of the conditions where the psychological work is placed in competition with the other sciences as elective courses in the great colleges; merely as a matter of self-preservation, psychology must gain the confidence of the students by offering lecture courses and laboratory exercises equal

[1] See Delabarre, *Les Laboratoires de Psychologie en Amérique*, "Annee Psychologique," 1894, i. 209.

in attractiveness and value to those of physics and chemistry. The modest and simple equipment with which the German investigator patiently produces his research-work, is quite inadequate for instructing large bodies of American students.

Passing to Asia, we find a large laboratory (Professor Motora) in the University of Tokio (Japan). Experimental instruction has just begun in the University of Pekin (China).

In conclusion, the future of experimental psychology seems a modest but assured one. It is now clearly understood that its methods are those of patient scientific work. It has not found—and cannot hope to find—an utterly new method of investigation or a startling principle of mental life; it cannot dissect the mind with a scalpel, nor has it discovered how to project thoughts along imaginary telegraph wires. Again, at the present day, it should not be expected to make discoveries of the interest and value of those repeatedly made by the other sciences with many well-trained men and more thoroughly equipped laboratories; many years of laborious development must ensue before discoveries equal in importance to those of argon and the X-rays can even be hoped for. The present firm establishment of our science in the leading universities, however, makes this development an assured one.

APPENDICES.

APPENDIX I.

VALUES OF THE PROBABILITY INTEGRAL $\frac{2}{\sqrt{\pi}}\int_0^{\gamma} e^{-t^2} dt$ FOR THE ARGUMENT γ.

γ	0	1	2	3	4	5	6	7	8	9
0.0	0.0000	0.0113	0.0226	0.0338	0.0451	0.0564	0.0676	0.0789	0.0901	0.1013
0.1	1125	1236	1348	1459	1569	1680	1790	1900	2009	2118
0.2	2227	2335	2443	2550	2657	2763	2869	2974	3079	3183
0.3	3286	3389	3491	3593	3694	3794	3893	3992	4090	4187
0.4	4284	4380	4475	4569	4662	4755	4847	4937	5027	5117
0.5	0.5205	0.5292	0.5379	0.5465	0.5549	0.5633	0.5716	0.5798	0.5879	0.5959
0.6	6039	6117	6194	6270	6346	6420	6494	6566	6638	6708
0.7	6778	6847	6914	6981	7047	7112	7175	7238	7300	7361
0.8	7421	7480	7538	7595	7651	7707	7761	7814	7867	7918
0.9	7969	8019	8068	8116	8163	8209	8254	8299	8342	8385
1.0	0.8427	0.8468	0.8508	0.8548	0.8586	0.8624	0.8661	0.8698	0.8733	0.8768
1.1	8802	8835	8868	8900	8931	8961	8991	9020	9048	9076
1.2	9103	9130	9155	9181	9205	9229	9252	9275	9297	9319
1.3	9340	9361	9381	9400	9419	9438	9456	9473	9490	9507
1.4	9523	9539	9554	9569	9583	9597	9611	9624	9637	9649
1.5	0.9661	0.9673	0.9684	0.9695	0.9706	0.9716	0.9726	0.9736	0.9745	0.9755
1.6	9763	9772	9780	9788	9796	9804	9811	9818	9825	9832
1.7	9838	9844	9850	9856	9861	9867	9872	9877	9882	9886
1.8	9891	9895	9899	9903	9907	9911	9915	9918	9922	9925
1.9	9928	9931	9934	9937	9939	9942	9944	9947	9949	9951
2.0	0.9953	0.9955	0.9957	0.9959	0.9961	0.9963	0.9964	0.9966	0.9967	0.9969
2.1	9970	9972	9973	9974	9975	9976	9977	9979	9980	9980
2.2	9981	9982	9983	9984	9985	9985	9986	9987	9987	9988
2.3	9989	9989	9990	9990	9991	9991	9992	9992	9992	9993
2.4	9993	9993	9994	9994	9994	9995	9995	9995	9995	9996
2.	0.9953	0.9970	0.9981	0.9989	0.9993	0.9996	0.9998	0.9999	0.9999	0.9999

This table, first published by Encke, is now to be found in nearly all works on probabilities or measurement. I have reproduced the compact form used in Merriman's "Textbook of Least Squares." More extended tables can be conveniently found in the following: (4 places), Kämpfe, "Phil. Stud.," 1893, ix. 145; (5 places), Meyer,

"Wahrscheinlichkeitsrechnung," Leipzig, 1879, (7 places), Weinstein, "Physikalische Maassbestimmungen," i. Berlin, 1880; Czuber, "Theorie der Beobachtungsfehler," Leipzig, 1891; (11 places), Bertrand, "Calcul des probabilités," Paris, 1889.

APPENDIX II.

SCHEMES FOR BERNOULLI'S THEOREM (PAGE 21).

The theorem may be thus stated: If two opposite events A and not-A occur according to the laws of chance, and if r and s are the times of occurrence required by those laws out of a total n, then, when n is large, the probability is

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2} dt + \frac{\sqrt{n}.e^{-\gamma^2}}{\sqrt{2\pi rs}}$$

that on a given occasion A will actually occur a number of times lying within $r \pm l$, where

$$l = \gamma \sqrt{\frac{2rs}{n}}.$$

The theorem may also be stated as follows. If p and q are the probabilities of the two opposite events A and not-A, it can be expected with a probability of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2} dt + \frac{e^{-\gamma^2}}{\sqrt{2\pi pqn}}$$

that the actual result for A will lie within $p \pm k$, where

$$k = \gamma \sqrt{\frac{2pq}{n}}.$$

Operations can be performed on thin paper laid over the page so that the scheme is visible. The quantities are as follows: n is the total number of cases; r is the number of cases of one kind (positive cases); s is the number of the other kind (negative cases); p and q are the probabilities of r and s, or $p = \frac{r}{n}$, $q = \frac{s}{n}$; l is the limit assigned to variation in r or s, and k the limit to variation in p or q. Σ indicates addition. $\square$ indicates the result after subtracting the upper number from the lower one. $\Phi(\gamma)$ indicates the first portion of the expression for P, and Ψ the last portion.

SCHEME WITH R, S, L.

$r =$	$\log r =$
$s =$	$\log s =$
$n =$	$\log n =$
$l =$	$\log l =$

$\log r =$	
$\log s =$	
$\log 2 = 0.3010$	
$\Sigma =$	
$\log n =$	
$\Delta =$	

$\frac{1}{2}\Delta =$	$= \log \frac{\gamma}{l}$
$\log l =$	
$\Sigma =$	$= \log \gamma$.
$\gamma =$	

$2 \log \gamma =$	
$\log \log e = 0.6378 - 1$	
$\Sigma =$.	$= \log \log e^{\gamma^2}$

$\log e^{\gamma^2} =$	
$\log \sqrt{\pi} = 0.2486$	
$\Sigma =$	
$\log \frac{\gamma}{l} =$	
$\Delta =$	$= \log \Psi$
$\Psi =$	
$\Phi(\gamma) =$	from table
$\Sigma =$	$= P$

SCHEME WITH P, Q, K.

$p =$	$\log p =$
$q =$	$\log q =$
$n =$	$\log n =$
$k =$	$\log k =$

$\log p =$	
$\log q =$	
$\log 2 = 0.3010$	
$\Sigma =$	
$\log n =$	
$\Delta =$	

$\frac{1}{2}\Delta =$	$= \log \frac{\gamma}{k}$
$\log k =$	
$\Sigma =$	$= \log \gamma$
$\gamma =$	

$2 \log \gamma =$	
$\log \log e = 0.6378 - 1$	
$\Sigma =$	$= \log \log e^{\gamma^2}$

$\log e^{\gamma^2} =$	
$\log n =$	
$\log \sqrt{\pi} = 0.2486$	
$\Sigma =$	
$\log \frac{\gamma}{k} =$	
$\Delta =$	$= \log \Psi$

$\Psi =$	
$\Phi(\gamma) =$	from table.
$\Sigma =$	$= P$

APPENDIX III.

THE MEDIAN.

Suppose that you have measured the heights of a group of twenty-five men, what do you do with the results, which vary, more or less, from one another? Let us take a case that occurs all over the country every year. Each graduating class of every college and school has the heights of its members measured. For the sake of comparison with preceding and succeeding classes, the individual heights are generally "averaged." Thus the "average" height of the class of '93 is said to have been, say, 66 inches, that of '94 67 inches, &c.

But a great deal of dissatisfaction arises with this "average" height. The presence of a single very tall or very short man upsets the whole result.

Let us take a class of ten men, of the heights of 65, 64, 65, 66, 66, 67, 65, 66, 76, 66 inches; the average height is 66.6 inches.

Now let us take another class, with the heights 65, 64, 65, 66, 66, 67, 65, 66, 66, 66. Every man except one has the same height as the corresponding man in the previous class, but there is no extraordinarily tall man there. The average is 65.6, or an inch less than that of the other class.

Now it is evident that the one very tall man ought not to outweigh the influence of the other nine men. It is apparent that the average is a very unsatisfactory representative value in such a case.

What is especially striking is that every man influences the result in direct proportion to his deviation from the average. For example, in the first class the man of 65 inches influences the average by the amount $\frac{66.6 - 65}{10} = 0.16$ of an inch, while the tall man influences it by $\frac{76 - 66.6}{10} = 0.94$ of an inch. This does not seem quite fair.

This difficulty and unfairness can be removed by treating every man alike, by allowing all measurements to affect the result to the same degree. This is done by the use of the median instead of the average.

The median was first considered as a representative value by Laplace, who treated some of its properties from a theoretical point of view. Fechner believed that it could be used for statistical purposes. For anthropometric measurements it has been made familiar by Francis Galton and by the investigations of the Anthropometric Committee of the British Association. Aside

from a few isolated attempts, the median was never practically used in purely psychological computation until I introduced it into my laboratory in 1893. During the year 1894 fully 100,000 measurements of the most varied kinds were adjusted on the basis of the median in place of or in addition to the average.

On the strength of this experience I have published ("Studies from the Yale Psychological Laboratory," vol. ii.) an analytical and practical consideration of the median and its properties.

What is the median? It is the middle value in a set of numbers. Suppose the numbers to be arranged in order of size; then, beginning at the smallest or at the largest, count them off until you come to the middle one. For example, the first class mentioned above would run 64, 65, 65, 65, 66, 66, 66, 66, 67, 76. The middle value for ten measurements would be between the fifth and the sixth. In this case, both are the same and the median is 66.

How much influence does the tall man have? Merely that of one man; as long as he is above 66 inches, it makes no difference how tall he is. In like manner every value influences the median merely as one value above or below. As tall men or short men are added, the median goes up or down according to the number of men added, not according to the number of inches added.

For the second class we had 64, 65, 65, 65, 66, 66, 66, 66, 66, 67. The median is 66.

The median for n values is that one which is the $\frac{n+1}{2}$th in order of size from either extreme. For five values it will be the the third, for thirty the 15½th, &c. It is not necessary to write the values in order of size; they can generally be selected by the eye. For short sets of values the whole process can be done mentally.

For example, suppose we have the following results of a set of experiments on reaction-time: 213, 215, 214, 210, 212, 214, 215, 210, 211. There are nine values, and the middle value is the $\frac{9+1}{2}$th or fifth. The smallest is 210, the next smallest is 210, the next 211, &c. The fifth is 213. The largest is 215, the next 215, &c. The fifth is 213.

Let us take, as another example, the set of values: 44, 51, 46, 50, 47, 49, 47, 45, 48, 50. The median will be the $\frac{10+1}{2}$th or 5½th value. The fifth from the smallest is 47; the fifth from the largest is 48; the 5½th will naturally lie between the two, and we take 47½ as the median.

A third variety of examples arises by taking a set of results where several values fall on the same number as the median. Suppose

we have 124, 123, 121, 123, 125, 124, 121, 120, 122, 123, 124, 123, 123, 122, 121, 125, 124, 121, 122.

Result.....	120	121	122	123	124	125
Times of occurrence.	1	4	3	5	4	2

The $\frac{19+1}{2}$th or tenth value is 123. The expression M = 123, however, does not mean M = 123.000..., but $123.5 > M > 122.5$, because we have been rounding off by errors of scale and errors of number all records to the unit-place instead of writing them to an indefinite number of decimals. Owing to the fact that five values fall upon 123, we know something about the first decimal place.

We are entitled to suppose that all records of 123 have arisen from values evenly distributed between 122.5 and 123.5 Counting from the smallest end, we have eight values up to the limit of the region covered by the middle values; counting from the larger end, we have six values down to the other limit of this region.

The general formula is expressed as follows: Let r be the value on which the median falls, and let there be m such values. Let the number of values above r be a and below r be b. Then take $c = a - b$ and

$$M = r + \frac{c}{2m}.$$

This will give the decimal places for the median.

In the last example $b = 8$, $a = 6$, $c = -2$, $m = 5$, $r = 123$, and consequently

$$M = 123 + \frac{-2}{10} = 122\,8.$$

Although we know something about the first decimal place, we know nothing of any worth regarding the second decimal place unless $m > 100$.

To make clearer the third kind of example, I subjoin a set of specimens that show how the changes in c affect the value M.

1. $r = 13$

11 12	13 13 13 13	14 15 15 15
$b = 2$	$m = 4$	$a = 4$
$c = 2$	$M = 13 + \frac{2}{8} = 13\frac{1}{4}$	

2.

$$\underbrace{11\ \ 12}_{\substack{b=2\\c=0}} \qquad \underbrace{\overbrace{13\ \ 13\ \ 13\ \ 13}^{r=13}}_{\substack{m=4\\M=13+0=13}} \qquad \underbrace{14\ \ 15}_{a=2}$$

3.

$$\underbrace{10\ \ 11\ \ 12}_{\substack{b=3\\c=-1}} \qquad \underbrace{\overbrace{13\ \ 13\ \ 13\ \ 13}^{r=13}}_{\substack{m=4\\M=13-\frac{1}{8}=12\frac{7}{8}}} \qquad \underbrace{14\ \ 15}_{a=2}$$

4.

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{\substack{b=4\\c=-3}} \qquad \underbrace{\overbrace{13\ \ 13\ \ 13\ \ 13}^{r=13}}_{\substack{m=4\\M=13-\frac{3}{8}=12\frac{5}{8}}} \qquad \underbrace{14}_{a=1}$$

5.

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{\substack{b=4\\c=-4}} \qquad \underbrace{\overbrace{13\ \ 13\ \ 13\ \ 13}^{r=13}}_{\substack{m=4\\M=13-\frac{4}{8}=12\frac{1}{2}}}$$

6.

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{\substack{b=4\\c=0}} \qquad \underbrace{13}_{\substack{m=1\\M=13}} \qquad \underbrace{14\ \ 15\ \ 15\ \ 15}_{a=4}$$

The computation in the last two kinds of examples is so simple that it can be performed with great rapidity. Those of the other kind are almost as easy as soon as the computer becomes familiar with the median. Indeed, the economy of the median, in respect to time, is something that can be appreciated only by one who is obliged to compute many sets of results.

Persons familiar with the science of measurement will realise the theoretical advantage of the value of middle probability (the median x_m) over the value of mean area (the average x_ξ), for all unsymmetrical curves of error (Fig. 121). x_h is the value of maximum probability. When the curve is symmetrical, the median and the average are the same.

Experience has shown that the median is often a better value than the average in many statistical and psychological measurements and in many other measurements where psychological factors come into play, *e.g.*, in the so-called errors of observation of astronomical and physical measurements. Strangely enough, such phenomena as the fluctuating price of corn or the varying records of the barometer are better represented by the median than by the average. Of course, in cases where the results are

reckoned by the number of units and not by the number of things, *e.g.*, meat by the pound, debts by the dollar, &c., the average is the proper value. (From the "Scientific American Supplement,"

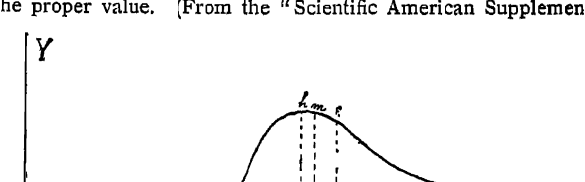


Fig. 121. RELATIONS OF MOST PROBABLE VALUE, MEDIAN AND AVERAGE, WITH AN UNSYMMETRICAL CURVE OF PROBABILITY.

1895, xl. 16,270 and the "Psychol. Rev.," 1895, ii. 376; the original and complete account is in "Studies from the Yale Psychological Laboratory," 1894, ii. 1)

APPENDIX IV.

LAMP BATTERIES.

For battery purposes the Yale Laboratory uses lamp batteries of my devising. The principle on which these are constructed is shown in Fig. 122. The plug at the end of the lamp cord is inserted into an ordinary lamp socket supplied with the city

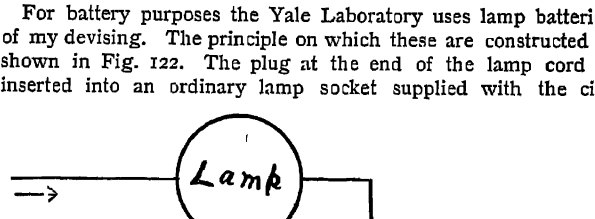


Fig. 122. PRINCIPLE OF THE LAMP BATTERY.

current. The large lamp, 32 c.p. or 50 c.p., allows 1 or 1½ amperes of current to pass from the main line to the apparatus. To eliminate the spark arising from the high voltage of the city current, a small 6 c.p. 12-volt lamp is placed in parallel with the apparatus. Whenever the apparatus circuit is broken, the current

passes through the small lamp. For larger currents more lamps are arranged in parallel, or larger lamps are used. An entire lampboard on this principle is shown in the frontispiece. Further details are to be found in vol. iv. of "Studies from the Yale Psychological Laboratory."

APPENDIX V.

ON THE MEASUREMENT OF IMAGINATION.

In the course of the investigations on measuring hallucinations it occurred to me that it might be possible to measure the intensity of an imagination also. The experiment was successful. The method is not difficult and is readily intelligible. In order to explain the method it will be sufficient to describe the first simple experiment made.

The apparatus consists of a screen of fine tissue paper. The tissue paper is illuminated by daylight in front and by a gas flame at the back. When the gas flame is turned down, the eye looking through a telescope tube sees a plain white circle illuminated by daylight.

The first experiment was made on a student accustomed to using the telescope. He was told to imagine hair lines on the white surface, like the hair lines seen in an astronomical telescope. This was successfully done. He was asked to describe them and compare their blackness. There is, he said, a horizontal line, which is the blackest of them, and three vertical lines of about equal blackness. He was told that the field of view was to be made gradually lighter by turning on a flame behind, and he was to tell how the lines behaved. As the gas was slowly turned on, he described various changes in the lines. Finally he said he saw a slant line that he had not imagined before. It appeared just about as black as the horizontal line and blacker than any of the others. Thereupon the experiment was ended.

The slant line was a real line. This he did not and still to-day does not know. On the back of the tissue paper a slant line had been drawn, and as the gas was turned up, of course it showed through. Thus we had a direct, unsuspecting comparison of intensity between a real line and an imaginary one.

The photometric determination of the intensity of the real line is not a difficult matter. A phantasimeter has been devised in which the graduation is done beforehand, but the simple arrangement just described serves to indicate the method of experiment. (From the "Scientific American," 1895, lxxii 85.)

APPENDIX VI.

COLOUR SIGHT TESTER (P. 58 AND CHAP. XXIV.).

The usual tests for colour blindness fail to detect those who are colour weak, although these persons are really colour blind for objects at a distance. For examinations of railroad employés and sailors I have invented a convenient instrument which not only detects the colour blind with rapidity and accuracy, but also detects the colour weak.

The use of different intensities of light for the purpose of a quantitative determination of colour blindness was, I believe, first made by Donders. In the form of a somewhat cumbersome lantern by Edridge-Green the test is now, I understand, officially adopted by the English Board of Trade.

In general appearance the colour sight tester resembles an ophthalmoscope. On the side toward the person tested (Fig. 123) there are three windows of glass one-quarter inch in diameter, numbered 1, 2, and 3 respectively. The opposite side of the tester (Fig. 124) consists of a movable disc carrying twelve glasses of different colours. As this disc is turned by the finger of the operater the various colours appear behind the three windows. At each movement of the disc the subject calls off the colours seen at the windows. The windows, 1, 2, and 3, are, however, fitted with grey glasses. No. 1 carries a very dark smoked glass; all colours seen through it will be dark. No. 2 carries a piece of ground glass, showing all colours in full brightness. No. 3 carries a light smoked glass. There are thus thirty-six possible combinations of the colours. The twelve glasses are, however, mainly reds, greens, and greys.

A suitable arrangement of the colours gives direct simultaneous comparisons of reds, greens, and greys of different shades. The well-known confusion by colour blind persons of dark greens with reds, greens with grey, &c., are exactly imitated, and the instrument gives a decisive test for colour blindness. Its peculiar advantage, however, lies in the fact that it presents reds, greens, and greys simultaneously in a large number of different shades of intensity. The light of a green lantern, as it appears to a colour weak person at different distances, is simulated by the red behind the different greys; at the same time a white light is also changed. The colour-weak person to whom weak green is the same as grey (white at a distance) is utterly confused and thinks that the weakened green is grey (white) and the dark grey is green.

The actual test is performed in the following manner: The tester is held towards a window, but not in the bright sunlight, at about 2½ feet from the person tested. The operator begins with any

chance position of the glasses, and asks the person tested to tell the colours seen through the three glasses, Nos. 1, 2, and 3. He answers, for example, "No. 1 is dark red ; No. 2 is grey ; No. 3 is green." The operator records from the back of the tester the

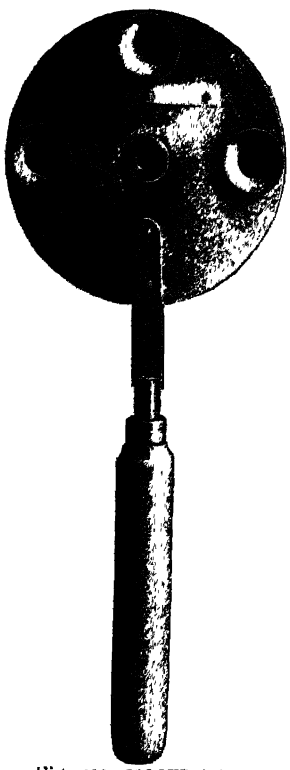


Fig. 123. COLOUR SIGHT TESTER (FRONT)

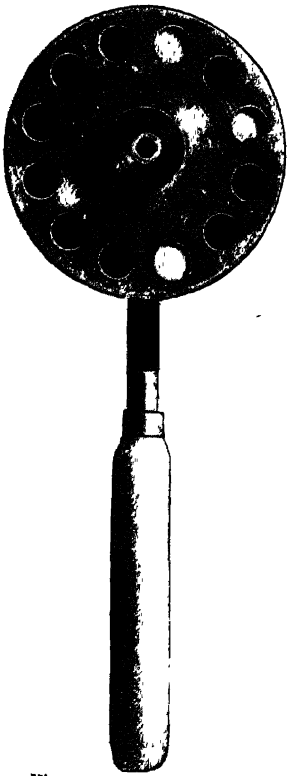


Fig 124. COLOUR SIGHT TESTER (BACK).

letters indicating what glasses were actually used. Suppose he finds that A, D, and G were opposite the glasses Nos 1, 2, and 3, he records · A 1, dark red ; D 2, grey ; G 3, green. The disc is then turned to some other position, the colours are again named, and the operator records the names used. For example, the result

might be: "No. 1 is dark green; No. 2 is white; No. 3 is red;" and the record would read: G 1, dark green; J 2, white; A 3, red. Still another record might give: J 1, dark grey; A 2 red; D 3, medium grey. Similar records are made for all combinations. Of course, the person tested knows nothing concerning the records made. The blank thus filled out is forwarded to the chief inspector of the railway or marine service. A comparison with a table containing the true colours for each position determines whether the test has been passed or not.

The records can be taken by any one, and, on the supposition that the record has been honestly obtained and that the instrument has not been tampered with after leaving the central office, the comparison is likewise mechanical. There is none of the skilful manipulation required in the wool test and none of the uncertainty attaching to its results. The only instruction given to the subject is, "Name the colours"; the results render the decision with mechanical certainty.

The three records just cited were all obtained from the red glass A, the grey glass D, the green glass G, and the white glass J, in combination with the dark grey No. 1, the white No. 2, and the medium grey No. 3. Those familiar with colour blindness will notice that these combinations place side by side the colours most confused. More important still is the fact that red, green, and white are made to undergo changes that detect the colour weak.

Another set of colours includes red, white, green, and blue-green, subject to all the combinations. For green I use the ordinary green glass common on most railways; the blue-green is known to dealers as "signal green," and is frequent on the water. The third set of colours is an additional test. It includes orange-brown, green, blue, and violet. These colours are confused in many cases of colour-weakness and colour-blindness.

APPENDIX VII.

FORMULAS FOR ADJUSTING MEASUREMENTS.

For those who may use this book in the laboratory I add a few formulas for the adjustment of measurements. Let the results of a set of n measurements of the same quantity under constant conditions be $x_1, x_2, \ldots, x_n$. The average is

$$a = \frac{x_1 + x_2 + \ldots + x_n}{n}$$

The individual variations (p. 47) or errors are $v_1 = x_1 - a$, $v_2 = x_2 - a, \ldots, v_n = x_n - a$. The mean variation, or mean error, is—

$$d = \frac{|v_1| + |v_2| + \ldots + |v_n|}{\sqrt{n\,(n-1)}}$$

where $|v|$ indicates that the sign of v is disregarded. For many purposes the following approximate formula (p. 47) can be used for calculating the mean variation (or average deviation)—

$$b = \frac{|v_1| + |v_2| + \ldots + |v_n|}{n}$$

A more reliable characteristic variation is the mean square error—

$$m = \sqrt{\frac{v_1^2 + v_2^2 + \ldots + v_n^2}{n-1}}$$

The most frequently used characteristic variation is the probable error—

$$r = 0.674\ m,$$

or approximately

$$r = \tfrac{2}{3}\ m.$$

The characteristic variations for the average of n measurements are

$$\mathrm{D} = \frac{d}{\sqrt{n}},\quad \mathrm{M} = \frac{m}{\sqrt{n}},\quad \mathrm{R} = \frac{r}{\sqrt{n}}.$$

In stating the results of a set of measurements, the average, one of the characteristic variations, d, m, and r, and either the number n or the characteristic for the series, D, M, or R, should be given. When, as is usually the case, several separate sets of measurements have been made on the same quantity, on different occasions, they may be lumped together or may be computed separately and then adjusted by weights. If the number of measurements is small, the only safe method is to average together all the single measurements made on the same quantity, unless the characteristic variations of the different sets are really very different. If the number of measurements is large, or if the conditions of the measurements change greatly for different sets, then the following method may be employed. The weights of sets of measurements are inversely proportional to the square roots of their characteristic variations. Thus, for a set of measurements with the variations, D, M, or R, the weight will be—

$$p = \frac{1}{\mathrm{D}^2},\ \text{or}\ p = \frac{1}{\mathrm{M}^2},\ \text{or}\ p = \frac{1}{\mathrm{R}^2}$$

For k sets of measurements with the separate averages a_1, a_2, ..., a_k, and the weights p_1, p_2, ..., p_k, the general average will be—

$$\mathrm{A} = \frac{p_1 a_1 + p_2 a_2 + \ldots + p_k a_k}{p_1 + p_2 + \ldots + p_k}$$

These few formulas will enable the experimenter to state simple results; for more complicated cases reference can be made to the works mentioned in Appendix I.

APPENDIX VIII.

FECHNER'S METHOD OF RIGHT AND WRONG CASES (P. 269).

Two stimuli differing by a small quantity D are compared together with the judgment "greater," "equal," or "less" for one of them as referred to the other. It is evident that the larger the difference D the greater will be the number r of "right" cases and smaller will be the numbers g and f of "equal" and "wrong" cases out of the total number n. As D decreases the proportion $\frac{r}{n}$ will also decrease, while $\frac{g}{n}$ and $\frac{f}{n}$ will increase. When there is no difference (D = 0), it is reasonable to suppose that $\frac{r}{n} = \frac{f}{n}$ ("right" and "wrong" being purely arbitrary terms in this case). Moreover, it would seem justifiable to divide the g cases into two parts and use $r' = r + \frac{g}{2}$ and $f' = f + \frac{g}{2}$ instead of r and f. This equal apportionment of the g cases is not very well justified except for D = 0, but can be retained for practical reasons. For D = 0, $\frac{r'}{n} = \frac{f'}{n} = \frac{1}{2}$. As D takes increasing values r' will increase and f' will correspondingly decrease by some quantity f(D) Thus $\frac{r'}{n} = \frac{1}{2} + f(D)$ and $\frac{f'}{n} = \frac{1}{2} - f(D)$. Since the conditions are those common to scientific measurements, we can assume that the usual laws of probability are valid whereby

$$f(D) = \frac{1}{\sqrt{\pi}} \int_0^{hD=t} e^{-t^2} dt,$$

where $e = 2.71828$ and h is the factor known as the measure of precision. When all errors of the apparatus and of manipulation are rendered negligible, h can be used as the measure of the subject's sensitiveness. A definite relation is thus established between the percentage of right cases, the actual difference and the sensitiveness. This relation is given in Fechner's table[1]:—

[1] Fechner, "Revision der Hauptpuncte der Psychophysik," 66, Leipzig, 1882.

$\frac{r'}{n}$	$t = hD$	$\frac{r'}{n}$	$t = hD$	$\frac{r'}{n}$	$t = hD$	$\frac{r'}{n}$	$t = hD$	$\frac{r'}{n}$	$t = hD$
0.51	0.0177	0.61	0.1975	0.71	0.3913	0 81	0.6208	0.91	0.9481
0.52	0.0355	0.62	0.2160	0.72	0.4121	0.82	0.6473	0.92	0.9936
0 53	0.0532	0.63	0.2347	0.73	0.4333	0.83	0.6747	0.93	1.0436
0.54	0.0710	0.64	0.2535	0.74	0.4549	0.84	0.7032	0.94	1.0994
0.55	0.0890	0.65	0.2725	0.75	0.4769	0.85	0.7329	0.95	1.1631
0.56	0.1068	0.66	0.2917	0.76	0.4994	0.86	0.7639	0.96	1.2379
0.57	0.1247	0.67	0.3111	0.77	0.5224	0.87	0.7965	0.97	1.3297
0.58	0.1428	0.68	0.3307	0.78	0.5460	0.88	0.8308	0.98	1.4522
0.59	0.1609	0.69	0.3506	0.79	0.5702	0.89	0.8673	0.99	1.6450
0.60	0.1791	0.70	0.3708	0.80	0.5951	0.90	0.9062	1.00	∞

Thus, if a difference of 2 grammes produces 62 °/₀ of r' cases, a difference of 4 grammes will for the same degree of sensitiveness produce 73 °/₀. Again, if with a given difference D = 2 grammes there are 62 °/₀ of r' cases for one person and 79 °/₀ for another, their degrees of sensitiveness bear the relation of 0.2160 to 0.5702 or 1 to 2.64. Finally, if a difference D = 2 grammes produces on one occasion 62 °/₀ of r' cases and a difference of D′ = 4 grammes produces on another occasion (under similar circumstances) 84 °/₀ of r' cases, the relation of the degrees of sensitiveness h and h' can be determined as follows:—For $\frac{r'}{n} = 0.62$, $hD = 0.2160$ and $h = 0.1080$; likewise for $\frac{r'}{n} = 0.84$, $h'D' = 0.7032$ and $h' = 0.1783$. Consequently $h : h' = 0.1080 : 0.1783 = 1 : 1.65$.

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